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# Our Bookshelf.

## Human Evolution.

*Éléments de sociologie : textes choisis et ordonnés.* Par Prof. C. Bouglé et J. Raffault. (Publications du Centre de Documentation sociale.) Pp. viii + 506. (Paris : Félix Alcan, 1926.) 30 francs.

A COLLECTION of extracts such as MM. Bouglé and Raffault have compiled has its manifest uses as a source book and work of reference. This, however, has not been the primary or even the main object of its preparation, although the course of the Écoles Normales has been followed in the chief features of its framework. It is intended as a manifesto and reply to those who maintain the unsuitability of sociology as a subject of instruction on the ground that it is too young a science to have become sufficiently systematised and adequately provided with principles of general application to afford a suitable discipline. The compilers desire to refute this criticism by submitting to the reader what sociological writers have actually said, instead of entering into abstract argument. In England, perhaps, we have been fortunate ; any controversy on this point being almost forgotten owing to an early acceptance of the evolutionary principle in its application to sociological data. Early exuberance, it is true, has had to be overcome, but the scientific attitude remains. This, however, does not affect the fact that for English readers, equally with French or those of any other tongue, this is a stimulating book in which the views of sociologists of to-day, yesterday, or long ago, on specific points arising out of a sociological scheme, may be found in their most characteristic form.

*Grundriss der Anthropologie.* Von Dr. M. W. Hauschild. Pp. viii + 235. (Berlin : Gebrüder Borntraeger, 1926.) 10·50 gold marks.

THE reviewer, after a perusal of this work, laid it down with the feeling that anthropology has suffered a profound loss by the death of its young author, Dr. Wolfgang Hauschild. In a preface written by his old teacher, Prof. Eugen Fischer, director of the Anthropological Institute of the University of Freiburg, we learn that Dr. Hauschild, after carrying out arduous investigations in Java during 1924, died from malaria on his way home, leaving behind him the manuscript of this work. One suspects that his text was intended to serve as a basis for a systematic course of lectures on the evolution and structure of human races, and that the condensation of his argument, although expressed in clear, simple German, would have been lightened and expanded by a free use of illustrative examples. It is plain, at least, that this rising anthropologist had addressed himself, not to the dilettante, but to the serious student of his subject.

The book is of particular interest to senior anthropologists, because it reveals the methods which the rising generation of investigators in Germany are applying to the problems of human evolution and of human heredity, and the ideals by which they are moved. Dr. Hauschild at once captures the English reader by ascribing to Darwin the just merit of being the sole author and initiator of the evolutionary movement which now moves thoughtful men in all parts of the world. He gives Mendel his due place—a very high one. The revolution in physical anthropology which Dr. Hauschild evidently anticipated was to arise from a fuller knowledge of the processes which regulate the development and growth of the human body. He has attempted to base his subject on physiological or biological principles, and for this reason has brought to bear on the human body the latest information gleaned from all branches of modern science. We see in this book a young author causing new sciences to leaven old knowledge, and all has been done in a logical, masterful way. We heartily commend this book to every one who wishes to get into touch with the best work now being done in the anthropological laboratories of Germany.

## Prehistory.

*Préhistoire de la Norvège.* Par Haakon Shetelig. (Instituttet for Sammenlignende Kulturforskning. Serie A : Forelesninger, 5.) Pp. vi + 280 + 10 planches. (Oslo : H. Aschehoug and Co. ; London : Williams and Norgate, Ltd. ; Leipzig : Otto Harrassowitz ; Paris : Honoré Champion ; Cambridge, Mass. : Harvard University Press, 1926.) 7*s.* 6*d.*

DR. SHETELIG, the distinguished Norwegian archæologist, some four years ago published a review of the state of knowledge of prehistoric Norway at that date. Unfortunately for most readers it was in Norwegian, and the present volume, though written on a slightly different plan, as well as covering a more extended field, will be welcomed by those who wish to make themselves acquainted with the views of this authority on the archæology

of his own country and its many problems. In his preface, Dr. Shetelig points out that the conditions, geographical and climatic, as well as the sparse population of early days, give the archæology of Norway a special character and, be it said, a peculiar significance. The salient problems throughout arise from the fact that the most marked characteristic of Norwegian culture is its receptivity and not its power of initiation. Norwegian culture, although individual, is a part of Scandinavian culture as a whole, and this in turn, although also distinctive, is an integral part of the general European culture. The task of the archæologist, therefore, is to analyse the data in such a way as to distinguish the foreign element and mark the modification by which it becomes essentially Norwegian.

The reader will follow with interest the method by which Dr. Shetelig has worked out this theme in tracing the development of Norwegian civilisation from the Maglemose period to the Viking age. In view of the use made of the early rock carvings in supporting the argument for the inheritance of palæolithic culture, as well as in the interests of the chapter on art, it is to be regretted that the book has not been more fully illustrated.

*Frühschein der Kultur: Bilder aus Vorgeschichte und Urzeit.* Von Prof. Dr. Johannes Ledroit. Pp. ix + 257. (Freiburg im Breisgau: Herder und Co. G.m.b.H., 1926.) 4·80 gold marks.

THIS well-planned little book aims at awakening the interest of the layman, and more especially of school-pupils, in prehistory by a lively and graphic presentation of the main facts. The outstanding features of each chief cultural period from Lower Palæolithic to Roman times are first briefly sketched. An imaginative description of some supposedly typical incidents from the epoch in question is added to fill in the details. We are introduced, for example, to a 'wandering artist' who winters in the cave of some reindeer-hunters and then passes on to another shelter after having adorned his hosts' abode with representations of bisons and other animals. We see the *Urgermanen* from the North Sea raiding the villages of the Michelsberg folk and witness the burial of a chief in a megalithic tomb. A description is given of a bronze age smithy and of the visit of a travelling dealer with copper and tin from England, who, with the aid of his 'slaves,' eventually carries off the smith's beautiful Nordic daughter.

Dr. Ledroit has carefully collected all the scattered scraps of archæological evidence that might help to complete a concrete picture of life in those remote times. There is less evidence of a mastery of the ethnographic data that might be invoked to supplement this inevitably fragmentary record, and a few mistakes have crept in. The bronze-worker may well have used the *cire perdue* process, but it is wrong to make him execute the spiral decoration on the finished casting; the mammoth is said to be a metre taller than existing elephants.

## Birds.

(1) *L'Évolution de l'ornithologie.* Par Dr. M. Boubier. (Nouvelle Collection scientifique.) Pp. v + 308. (Paris: Félix Alcan, 1925.) 10 francs.

(2) *Birds and their Attributes.* By Dr. G. M. Allen. Pp. xiii + 338 + 34 plates. (London, Calcutta and Sydney: George G. Harrap and Co., Ltd., 1926.) 10*s.* 6*d.* net.

(3) *Les oiseaux: l'ornithologie et ses bases scientifiques.* Par Dr. M. Boubier. (Encyclopédie scientifique: Bibliothèque de zoologie.) Pp. iv + 305. (Paris: Gaston Doin et Cie., 1926.) 22 francs.

(4) *Fugle.* 2: *Lomfugle, Stormfugle, Vandhøns, Tranefugle og Vadefugle.* Ag R. Hørring. (Danmarks Fauna, 30.) Pp. 332. (København: G. E. C. Gads Forlag, 1926.) n.p.

(1) IN the steady stream of books about birds it is pleasant to find one which breaks new ground, as does Dr. Boubier's "L'Évolution de l'ornithologie." In a series of eight chapters he takes up particular aspects of ornithology and traces the gradual growth of knowledge in each branch of the science, marking the stages of development by appeal to the works of the masters. Thus he discusses the knowledge of the European avifauna, the results of explorations and voyages, the study of migration, of taxonomy, anatomy, and palæontology, and the development of systems of classification, and under each head the history is traced succinctly yet with sufficient detail to give a connected picture of progress. In so comprehensive a work omissions are bound to occur, and we note that no English writer on bird migration is mentioned later than Barrington (1772), and that although many and lesser bird-ringing schemes are referred to, the *British Birds* scheme which has been responsible for the marking of some 146,000 birds passes unnoticed.

(2) Dr. Allen's volume is excellent, although it belongs to a very familiar type—an introduction to a general survey of birds, their structure, habits, and human relations. It is outstanding because of the author's familiarity with recent literature, and his knack in selecting a telling example, as in the case of the narrow Atlantic belt which bars the distribution of two sets of terns (p. 155), or the Ceylon telegraph wire which bore an annual crop of mistletoe seedlings (p. 165), or the relation of the extent of a bird's migration to the shape of the egg it lays (p. 177). Of the illustrations the most remarkable are Mr. Bigelow's photographs of the V-shaped flight formations of swans and geese.

(3) In this other work of Dr. Boubier's we have a general treatise on birds, aiming less at the general reader than at the ornithologist who would add a scientific ground-work to his own observations. It is noteworthy for its conciseness, and for the care with which the comparative anatomy of birds is treated. A number of rough but very instructive diagrams illustrate the text.

(4) This, the second volume of a handbook of the birds of Denmark, deals with the divers, grebes, petrels, rails, and waders. The characters of the various species are indicated in key-tables, summer and winter plumages are described, a lengthy section gives an account of the migrations, habits, and status in Denmark of each species, and each description is accompanied by an excellent illustration in half-tone of the bird itself. J. R.

## Systematic Zoology.

*Handbuch der Zoologie: eine Naturgeschichte der Stämme des Tierreiches.* Gegründet von Prof. Dr. Willy Kükenthal. Herausgegeben von Dr. Thilo Krumbach. Vierter Band: *Progoneata, Chilopoda, Insecta.* Erste Lieferung. Pp. 128. 13 gold marks. Zweite Lieferung. Pp. 129-240. n.p. Fünfter Band: *Solenogastres, Mollusca, Echinodermata, Tunicata.* Dritte Lieferung. Pp. 177-256. 8 gold marks. (Berlin und Leipzig: Walter de Gruyter und Co., 1926.)

THE fourth volume of Kükenthal's treatise opens with a short explanation of the extinction of the term Myriapoda, which has lost its strict systematic significance.

After a definition of the class Progoneata, the author, Dr. Carl Graf Attems, passes to the consideration of the constituent subclasses — Symphyla, Pauropoda, and Diplopoda—their anatomy, developmental stages, ecology, distribution, and classification. The sections on the Symphyla and Pauropoda are short—each about 9 pages—but that on the Diplopoda extends to more than 200 pages, and gives an admirably complete account of this subclass and its affinities. The classification is done in great detail—into orders and then into genera. The space devoted to systematics is disproportionately large, and we hope it may not result in restricting the pages available for the account of the Insecta in the later part of the volume. The parts are well illustrated, and at the end of the account of each class is a list of works of reference.

The first quarter of section 3 of vol. 5 contains the conclusion of the account—the development, ecology, and classification—of the Bivalvia. The rest of the part is devoted to the Cephalopoda, the description of which by Prof. J. Thiele is on the lines of the two preceding parts of the volume already noticed in NATURE. He gives a fair account of the anatomy, but finer structural details receive insufficient consideration, and reference to some of the newer work is lacking, for example, to the recent views on symbiotic organisms in relation to the luminous organs, to the details of formation and discharge of the remarkable spermatophores, and to the young stages of Spirula.

More illustrations would have been welcome in this section; particularly of important aspects such as the egg-cleavage and later stages of development, and the histology of the statocysts, eyes, and chromatophores.

*Die Tierwelt der Nord- und Ostsee.* Herausgegeben von G. Grimpe und E. Wagler. Lieferung 5. Teil 9.$d_1$: *Lamellibranchia,* von F. Haas; Teil 12.$a_1$: *Copelata,* von A. Bückmann. Pp. 96 + 20. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1926.) 8·80 gold marks.

THESE new parts of "Die Tierwelt der Nord- und Ostsee" maintain the high standard set by those already published. The greater part consists of a well-balanced account of the lamellibranchs by F. Haas; it is perhaps to be regretted that he uses the older classification based mainly on the teeth and hinge of the shell, rather than the newer classification by means of the gill structure, but he provides what will be of great service to the marine biologist, namely, good keys to the genera and species. There is an interesting account, illustrated with good tables, of the distribution of all the species and, in addition, a very brief summary of the present state of our knowledge regarding their structure and physiology is included.

The bionomics of the lamellibranchs—a subject of the utmost importance—are given due prominence by the author, both from the ecological and utilitarian points of view. Limnoria, however, is better considered as the precursor than as the enemy of Teredo, although the author admits that if it increased at the expense of Teredo "Teufel durch Beelzebub ausgetrieben würde."

The concluding pages of the parts before us are devoted to a short and very clearly illustrated account of the Copelata (Appendicularіæ), which has been contributed by A. Bückmann.

*Les arachnides de France.* Par Eugène Simon. Tome sixième. Deuxième partie: *Le synopsis général et le catalogue des espèces françaises de l'ordre des Araneæ (suite).* Œuvre posthume publiée par L. Berland et L. Fage. Pp. ii + 309-532. (Paris: L. Mulo, 1926.) 25 francs.

THIS part does not complete the supplement to "Les Arachnides de France." It deals with two sub-families of the Argiopidæ—the Theridiosomatinæ and Erigoninæ. The former contains the genus Theridiosoma, which is in some respects a link between the Theridiidæ and Argiopidæ and is represented in France and Great Britain by one species only. In the Erigoninæ thirteen new genera have been created without the addition of any new species, which seems somewhat drastic treatment. Some of the generic distinctions are slight and the generic characters will in some cases be more difficult to determine than the specific ones.

This part will be something of a disappointment to British arachnologists, who have been anxiously awaiting the remainder of this volume in the hope that the British and European classification of spiders might once and for all be unified. It is certainly a valuable contribution but falls short of our hopes.

*Grundriss der allgemeinen Zoologie: für Studierende.* Von Prof. Dr. Alfred Kühn. Zweite, verbesserte und vermehrte Auflage. Pp. viii+261. (Leipzig: Georg Thieme, 1926.) 13·20 gold marks.

In the first 84 pages the author gives an account, necessarily very condensed, of the various phyla. It is difficult to estimate whether some of the sections, *e.g.* one page of text for the sponges, about ten lines for the Ctenophora, six lines for the Bryozoa and for the Myriapoda respectively, will serve to convey to the student an adequate idea of the groups concerned. The author may depend largely on the laboratory instruction for the building up of the student's knowledge of structure, and the brief accounts may be intended chiefly for purposes of revision. The second part of the work (about 70 pages) deals with physiology—food and its transportation in the body, production of energy, respiration, excretion, animal heat, chemical relations between the organs, movement, reactions to stimuli. The final section of about 80 pages is devoted to embryology, heredity, and the origin of species. The figures are diagrammatic and clear; there is a list of works of reference and an adequate index.

## Special Biological Studies.

*Evolution im Lichte der Bastardierung betrachtet.* Von J. P. Lotsy. Aus dem Englischen übersetzt von H. N. Kooiman. (Separat Abdruck aus *Genetica*, 7.) Pp. iii+365-470. (Haag: Martinus Nijhoff, 1926.) 4 guilders.

Early in 1925, Dr. J. P. Lotsy, at the invitation of the university colleges of New Zealand, delivered there three lectures on "Evolution considered in the Light of Hybridisation." These were afterwards published by Canterbury College, together with an introduction and an appendix by Dr. L. Cockayne, giving a list of more than 200 supposed wild hybrids in the New Zealand flora. These lectures have now been translated into German by Dr. H. N. Kooiman. The original edition was illustrated by eleven photographs of Mirabilis hybrids. These are replaced in the German edition by a coloured plate illustrating the remarkable range of fruit types obtained in the $F_2$ and $F_3$ generations from a cross between two varieties of pumpkin or squash.

So early as 1921, Dr. Cockayne had observed natural hybrids of Nothofagus and other genera growing wild in profusion, and he gives reasons why New Zealand is a particularly suitable place for their evolutionary study. Nevertheless, if natural species-hybrids are so abundant in the New Zealand flora, they must be of common occurrence elsewhere. In polymorphic genera, such as Rubus, Cratægus, Rosa, and Hieracium, the polymorphism is now, as the result particularly of cytological work, generally recognised to have arisen in connexion with crossing, and the number of such cases will no doubt be much extended.

Dr. Lotsy in his lectures has brought together many new and interesting data bearing on the subject. The statement of his views is less extreme than formerly, but he still leans very heavily upon the hybridisation hypothesis in places where it seems unnecessary. Although he admits that real mutations may take place, yet he invents terms such as 'sub-haploid' and 'super-haploid' hybridisation for processes with which geneticists have long been familiar as germinal changes, *e.g.* various processes by which new chromosome numbers arise. Why he assumes that such changes can only occur in Nature as a result of crossing it is difficult to see, since it is well known that some of them at least can be produced experimentally by changes in the conditions. Dr. Lotsy's view appears at its weakest in dealing with adaptations. Thus he suggests that the leafless Cacti and Euphorbiae may have arisen by the crossing of leafy succulent forms, the leafless offspring afterwards finding their way into the desert.

We think the author would strengthen his case if instead of trying to make hybridisation a universal evolutionary factor, he recognised it as merely one of the conditions under which the evolution of sexual organisms has taken place. He thinks evolution was made possible by sexuality, but he makes no attempt to explain the evolution of non-sexual organisms. R. R. G.

*Der Formwechsel der Protistenkerne: eine vergleichend-morphologische Studie.* Von Dr. Karl Bělař. (Sonderabdruck aus *Ergebnisse und Fortschritte der Zoologie*, Band 6.) Pp. 420. (Jena: Gustav Fischer, 1926.) 22 gold marks.

The author has made a thorough study not only of the literature of the Protozoan nucleus, but also of numerous specimens in various phases of nuclear activity. As he remarks, the newest developments of research in heredity have brought the nuclear changes in the Metazoa into the very centre of interest, but in the Protozoa genetical studies are only beginning, and much remains to be accomplished in this and in other connexions in the study of their nuclei. It is well, therefore, that the known facts have been brought together in such an accurate and orderly manner, for the limitations of our knowledge and the directions in which further investigations are required are more readily realised.

After a survey of the 'resting' nucleus, the author passes to the consideration of the various forms of mitosis and amitosis. He points out that amitotic nuclear division, formerly believed to be widespread in the Protozoa, appears normally to occur in the macronucleus of the Ciliata and in a few other cases only, and most of the latter are doubtful. The more interesting examples are carefully described.

The account of the behaviour of the nucleus preparatory to and during fertilisation includes a summary of present knowledge of the meiotic phase. A list is given of the number of chromosomes in nearly one hundred Protozoa, in some twenty of which the diploid and haploid numbers are stated. The nuclear changes which take place during the vegetative period are traced, the rela-

tionships between the nucleus and the cytoplasm and other components of the organism, *e.g.* flagella, cilia, trichocysts, are discussed and the spindle and centrosome are described. The last chapter is devoted to the more important views on the constitution of the protozoan nucleus, and includes a series of diagrammatic figures representing the different modes of division of the nucleus in the various groups of Protozoa.

In order not to burden the text with quotations from authors, Dr. Bělař has placed these, together with his remarks on them, in an appendix of some sixty pages. There is an extensive list of references and an index to the genera and species mentioned in the volume.

The accurate and critical treatment of this difficult subject has resulted in a volume which will be of great use to those who contemplate the investigation of the nucleus not only of the protozoa but also of the fungi and of the algæ. Special praise is due to the author and to the publisher for the abundance and excellence of the illustrations (many of them original) and for the detailed legend with which each is provided.

*Die Pflanzenareale: Sammlung kartographischer Darstellungen von Verbreitungsbezirken der lebenden und fossilen Pflanzen-Familien, -Gattungen und -Arten.* Unter Mitwirkung von Prof. Dr. Ludwig Diels und Prof. Dr. G. Samuelsson. Herausgegeben von Prof. Dr. E. Hannig und Prof. Dr. H. Winkler. 1 Reihe, Heft 1. Pp. iii + 14 + 10 Karten. Heft 2. Pp. 19-34 + 11 - 20 Karten. (Jena: Gustav Fischer, 1926.) 7.50 gold marks.

THAT the study of the distribution of plants is becoming of increasing importance in systematic work is being more and more realised. The controversial theories put forward by Willis (age and area) and by Wegener (continental drift), even if they have done nothing else, have been the means of stimulating a new interest in this branch of botany. A comprehensive work on the subject has now been commenced in Germany under the above title, edited by Drs. E. Hannig (Munster) and H. Winkler (Breslau), with the assistance of Drs. L. Diels (Berlin) and G. Samuelsson (Stockholm). The two parts issued are very large quarto, similar in appearance to the well-known "Vegetationsbilder" of Karsten and Schenck.

In Part 1 Prof. Engler gives an account of the distribution of Saxifraga, subsection Hirculoidea, to which are devoted three large maps. Other authors deal with the distribution of various genera and species of plants in a similar manner, amongst them *Genista anglica*.

In Part 2 Prof. H. Winkler shows the area occupied by the banana family, Musaceæ, in a wild state, and another map indicates graphically the range of the species of Musa under cultivation. Prof. Pax gives a short account of the genus Sapium and shows the distribution of the sections proposed by him in "Das Pflanzenreich." Other botanists deal with the range of various trees. In the case of the European and Mediterranean species of Abies, Dr. Mattfeld gives no less than two full pages of bibliographical references, in contrast to Prof. Pax's three references for Sapium. The work is somewhat unwieldy, resulting in considerable waste of space. For example, Karte 2 shows the distribution of three species of Saxifraga in a very small area of Central Asia on a circumpolar map about 10 in. × 14 in. A more judicious selection of sectional maps might easily have reduced the first part to at least half of its present proportions.

## Polarised Light in Biology.

(1) *Handbuch der biologischen Arbeitsmethoden.* Herausgegeben von Prof. Dr. Emil Abderhalden. Lieferung 191. Abt. 2: *Physikalische Methoden*, Teil 2, Heft 2. *Die Verwendung der Polarisationsmikroskops für biologische Untersuchungen.* Von August Köhler. Pp. 907-1108. (Berlin und Wien: Urban und Schwarzenberg, 1926.) 9·30 gold marks.

(2) *Das Polarisationsmikroskop: seine Anwendung in der Kolloidforschung und in der Färberei.* Von Hermann Ambronn und Albert Frey. (Kolloidforschung in Einzeldarstellungen, Band 5.) Pp. x + 195. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1926.) 13.50 gold marks.

THESE volumes represent recent additions to two series of 'handbooks' dealing with experimental methods in biology and the study of colloids respectively. The polarisation microscope, once finding its application exclusively in orthodox crystallography, is now being applied to the study of what Sir William Bragg so expressively terms "the imperfect crystallisation of common things."

The highly regular molecular arrangements often found in such objects as vegetable fibres, nerve filaments, cell walls, and so on, frequently leads to double refraction of large amount, which is measured without difficulty by the highly refined methods originally developed for mineralogical purposes.

(1) Köhler's discussion is arranged largely as a series of experiments designed to illustrate the main applications of the polarisation microscope, the determination of the directions of the axes of the index ellipse (the 'fast' and 'slow' directions), the use of the Sénarmont compensation systems, etc., all being thoroughly explained on physical principles with the minimum of mathematics. The optical features of konoscopic methods are also carefully treated, and a number of representative biological and colloidal applications are discussed. Unfortunately, the separate volumes of this series are not given an individual index, which somewhat detracts from the utility of each section by itself, but it is meant to be read in conjunction with Köhler's previously issued volumes on the microscope in the same series, and will be found clear and adequate.

(2) The volume by Ambronn and Frey will be found useful for readers more familiar with physics, and covers the ground rather more rapidly than the volume above. The latter half of the book is

devoted to the physical applications of the various methods; a valuable section deals with the results of O. Wiener's "Theorie des Mischkörpers," another with liquid anisotropism, and so on. Part 3 deals with the various optical methods employed to glean information as to the structure of dispersoids, and the directions of the particles. Celluloid and rubber are among the substances specifically discussed. The name of Richard Zsigmondy, the editor of the series, will be a guarantee of the reliability of this 'handbook,' which naturally explains much of the recent work of the authors.

*Handbuch der biologischen Arbeitsmethoden.* Herausgegeben von Prof. Dr. Emil Abderhalden. Lieferung 205. Abt. 2: *Physikalische Methoden*, Teil 2, Heft 4. *Polarimetrie*, von Heinrich Kessler; *Spektroskopische Methoden des Mediziners*, von Fritz Löwe; *Nephelometrie*, von Marie Anna Schirmann. Pp. 1345-1536. (Berlin und Wien: Urban und Schwarzenberg, 1926.) 8·70 gold marks.

FULL theoretical accounts of the subjects of polarimetry, spectroscopy, and nephelometry are given, together with descriptions of selected pieces of apparatus. The accounts are written from the point of view of the biologist rather than the physicist, especially the section dealing with spectroscopy, in which special reference is made to the blood pigments. The work should be consulted by all those interested in these particular branches of biological methods.

## Modern Physics.

*Die elektrische Leitfähigkeit der Atmosphäre und ihre Ursachen.* Von Prof. Dr. Victor F. Hess. (Sammlung Vieweg, Heft 84-85.) Pp. viii + 174. (Braunschweig: Friedr. Vieweg und Sohn A.-G., 1926.) 8.50 gold marks.

As at once a professor of experimental physics and a field observer, Dr. V. F. Hess is specially fitted to write a general text-book on atmospheric electricity, and we learn from the preface that a work of this character by himself and his colleague at the University of Gratz, Prof. H. Benndorf, is in preparation. The present work deals with only part of the subject, and even within its ostensible limits it is not apparently regarded as altogether exhaustive. On one or two points the more extensive work in contemplation is referred to for further information, and in particular it will supplement the bibliography of the present work, though that is fairly extensive.

In the main the book treats of the electrical charges in the atmosphere and their origin. The theory of the Gerdien and Ebert apparatus is discussed, and the various methods of investigating the radioactive phenomena of the atmosphere. Various allied questions, *e.g.* the electricity of rain and thunderstorms, are only lightly touched on. As a pioneer investigator of the penetrating 'cosmic' radiation, Dr. Hess naturally gives special attention to that subject. He traces its development from the independent discovery by Gockel and C. T. R. Wilson of the ionisation in closed vessels, followed by the field work of McLennan, Wright, and others, then by the balloon observations by Gockel, Kolhörster, and himself, and finally by the recent work of Kolhörster and Millikan. He objects not unreasonably to the term 'Millikan rays' used by some Americans, but in his references to the conductivity of the upper atmosphere—a subject on which he supplies most up-to-date information—he makes free use of the term Kennelly-Heaviside layer, to which like exception can be taken.

The book so far as it goes may claim to replace the earlier German text-books by Gockel and by Mache and v. Schweidler, and within its sphere it is not inferior to the recent French works. It is interesting to note that in the name index at the end, the references to Elster and Geitel are only as numerous as those to Kolhörster, and only half as numerous as those to Prof. W. F. G. Swann. Dr. Hess does not, however, seem to notice that if the somewhat serious defect in the Ebert apparatus which he accepts as proved by Prof. Swann really exists, then much of the information which appears in the present book and elsewhere respecting negative ions must require correction. The view is taken that if satisfactory results are to be obtained in the open, the Ebert apparatus must be surrounded by a large mesh earth-connected wire screen. This seems a matter which deserves international consideration. C. CHREE.

*Müller-Pouillets Lehrbuch der Physik.* Elfte Auflage. Herausgegeben von A. Eucken, O. Lummer, und E. Waetzmann. In fünf Bänden. (1) Band 2: *Lehre von der strahlenden Energie (Optik)*. Erste Hälfte. Bearbeitet von O. Lummer. Pp. xviii + 928 + 7 Tafeln. 50 gold marks. (2) Band 3: *Wärmelehre*. Erste Hälfte: *Physikalische chemische und technische Thermodynamik (einschl. Wärmeleitung)*. Bearbeitet von Arnold Eucken. Pp. xviii + 1185. 63 gold marks. (Braunschweig: Friedr. Vieweg und Sohn A.-G., 1926.)

THIS eleventh edition of one of the best-known German treatises on physics is the first that has appeared since the War. It shows conclusively that Germany is neither forced nor willing to abdicate her former leading position in the matter of exhaustive text-books.

(1) This volume may be regarded as Dr. Otto Lummer's last legacy to humanity. It was already in type when he died in July 1925, and has been finally brought out by A. Eucken and E. Waetzmann. It is significant of modern developments that the conception of an ether of space, which in the late pre-War years seemed to be losing ground, has to some extent been rehabilitated by the generalised theory of relativity, and particularly by the system worked out by Weyl in 1918. These theories regard the electromagnetic field and the gravitational field as two aspects of the 'metric' field which comprises all phenomena of time and space. The old contrast between

matter and ether is eliminated by the conception of matter as condensed and 'grained' energy in the gravi-electromagnetic ether.

The present volume covers the same ground as the corresponding volume of the tenth edition, except that spectrum analysis and phosphorescence are relegated to a further volume. The chapters on the eye and on optical instruments are entirely rewritten, and there are many additions to the chapter on interference and (especially) on prism combinations.

(2) The third volume, which deals with heat, has been subdivided, and while the book under notice deals with thermal phenomena in general, the next subdivision will deal exclusively with the kinetic theory.

It is literally true that the whole of the work has been rewritten. In this particular volume we find none of the old names. Pfaundler, Drucker, Wassmuth, and Hann are replaced by Eucken, Ebbecke, Jakob, Magnus, and others, and the bulk of the work has been done by Eucken, who celebrated his eightieth birthday this year!

It suffices to say that the new editors have carried out their great task in the spirit and tradition of the original work. No greater praise is necessary. "Müller-Pouillet" still stands unrivalled among 'elementary' text-books of physics which aim at being at the same time clear, accurate, and complete.

*Anregung von Quantensprüngen durch Stösse.* Von Prof. Dr. J. Franck und Dr. P. Jordan. (Struktur der Materie in Einzeldarstellungen, Band 3.) Pp. viii + 312. (Berlin: Julius Springer, 1926.) 19·50 gold marks.

THIS book is an up-to-date account of the work done in recent years on this interesting and fertile subject, a field of inquiry which is associated with the names of Franck, Hertz, Paschen, and Heisenberg in Germany, of Fowler and Horton in Great Britain, and of numerous contributors to the *Physical Review* in America. The first chapters describe and discuss the kinetics of slow-moving electrons in gases and vapours, the determination of critical potentials by the electron impact method and the relations between critical potentials and the spectral terms of atoms. Succeeding chapters discuss in detail the possibilities of excitation and of ionisation of atoms by electron impacts, and give a well-balanced account of collisions 'of the second kind.' In the seventh chapter an account is given of the critical potentials of molecules, especially those of hydrogen, nitrogen, oxygen, and the halogens. The concluding chapter deals briefly with the applications of the results obtained to chemical reactions.

From the first experimental work of Franck and Hertz to the latest theoretical speculations of Heisenberg is indeed a far cry. Perusal of this book will convince a reader to what extraordinary ramifications of physics the discovery of the electron and the application of quantum ideas to the structure of the atom have led. The subject is a difficult one; difficult both in its fundamental concepts and in the manner in which these are expressed, but one which every physicist must, and every physical chemist should, attempt to understand. It is a great boon, therefore, to have this up-to-date, fully documented compilation in the well-digested form which Prof. Franck and Dr. Jordan have given us. Both the experimental and theoretical sides of the work are adequately treated. To many this book will make a difficult subject reasonably clear and simple for the first time; it is a book which we whole-heartedly recommend to all students of physics.

## Chemistry and Colour.

*La chimie des matières colorantes organiques.* Par Dr. Pierre Castan. (Encyclopédie scientifique: Bibliothèque de chimie.) Pp. 456. (Paris: Gaston Doin et Cie, 1926.) 30 francs.

THE editor of the "Chemical Library," of which this book forms a unit, does not aim at producing dictionaries or books of reference, but books that will be *read* not by the chemist alone, but also by those who are interested in other branches of science, and by the general reader. The complete library will contain some eight volumes on general chemistry, twelve on inorganic chemistry, and eleven on organic chemistry, of which this volume is the second to appear.

The author is to be congratulated on having produced a most readable book, of which the opening chapter on the relation between the colour and constitution of organic compounds is a remarkably clear exposition of this important problem. In the succeeding eight chapters the chemistry of the different classes of synthetic dyestuffs is discussed, the final chapter being devoted to the naturally occurring colouring matters. Within the limited scope of the present volume there is no place for a section dealing with the chemistry of dyestuff intermediates; it is to be hoped that this serious omission will be rectified in some succeeding volume of the series.

*Pyrosole: das kolloide Phänomen in der glühend Flüssigen Materie und seine Erstarrungszustände.* Unter Berücksichtigung des latenten photographischen Bildes. Von Prof. Dr. Richard Lorenz und Prof. Dr. Wilhelm Eitel. (Kolloidforschung in Einzeldarstellungen, Band 4.) Pp. ix + 290 + 20 Tafeln. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1926.) 20 gold marks.

COLLOIDAL solutions in which the continuous medium is solid at ordinary temperature are common in industrial processes, but owing to the experimental difficulties of investigating them, they have received less attention than solid-liquid systems. Prof. R. Lorenz is a pioneer in this field, and the monograph gives a summary of our knowledge of metal fogs in fused salts, coloured glasses, and the colloidal materials in minerals and slags. The consideration of similar solid colloidal solutions formed by the action of light leads naturally to an account of modern work on the

photohalides and the latent photographic image. The excellent and numerous illustrations assist in making the book exceptionally interesting and useful.

## Miscellany.

*Tabulæ Biologicæ.* Ed. W. Junk. Herausgegeben von C. Oppenheimer und L. Pincussen. Band 3: *Allgemeine Chemie, Konstanten chemischer Verbindungen, Chemie der Organe, Stoffwechsel, Grösse und Massen-Verhältnisse beim Menschen, Pharmakologie, Immunität.* Pp. vi + 829. (Berlin: W. Junk, 1926.) 63*s.*

THE contents of the third volume of "Tabulæ Biologicæ," which we have recently received, are somewhat inadequately indicated by the short titles of the different sections, given above. A vast amount of information of interest to physiologists and biochemists may be found here, which, in general, appears to be adequate and up-to-date. Thus the formulæ and chemical and physical constants of all the better-known substances on which physiologists and pharmacologists may require information are given in this volume; data are provided on the different natural and synthetic drugs used in medicine, including a most useful table of the innocuous, toxic, and lethal doses of a great variety of compounds. This volume also contains data on the chemistry of different organs and on metabolism; in the latter section may be found such information as the oxygen and sugar consumption of different tissues, the heat value and composition of different foodstuffs, and the physical constants of man himself at all ages. Whilst, as is perhaps inevitable in a work of this kind, equal stress is laid on data which may be of unequal value owing to the differences in the number of experiments on which the figures are based, and whilst in places the data themselves appear inadequate, as, for example, the table on the vitamin content of different foods, yet our thanks are due to the editor and his collaborators for providing biologists with such readily accessible information on a wide variety of subjects, culled from so many different sources.

*Die Kriegsschauplätze 1914–1918 geologisch dargestellt.* Herausgegeben von Prof. Dr. J. Wilser. In 14 Heften. Heft 14: *Die Isthmuswüste und Palästina.* Von Dr. Paul Range. Mit einem Beitrage von Dr. Walter Hoppe: *Paläontologie und Paläogeographie der Jura- und Kreideschichten der Isthmuswüste.* Pp. vi + 82. (Berlin: Gebrüder Borntraeger, 1926.) 11·40 gold marks.

DURING the War, Dr. P. Range was attached to the German-Turkish forces operating in Palestine to advise on the important matter of water-supply, and so had the opportunity of making a fairly detailed geological reconnaissance of the coastal plain of Palestine and the desert of northern Sinai (the Isthmus Desert) to the south. His chief observations have already been published in a number of papers in German periodicals, and include the discovery of important exposures of Jurassic rocks in the Maghara Range, in northern Sinai—a discovery in which, however, he had been anticipated by a Frenchman, M. Couyat-Barthoux, a year or two previously.

Since the War, a number of important papers on the geology of Palestine have appeared from other quarters also, and it is gratifying to note that the recent important discoveries of marine Trias and Jura in nearer Transjordania have been made by British geologists.

In including an account of these recent advances the present volume fulfils a useful purpose. It rather supplements than replaces the section on Palestine in Prof. M. Blanckenhorn's volume, "Syrien, Arabien, und Mesopotamien," published in 1914 in the "Handbuch der regionalen Geologie." Very little is said about the structural features of the region and their history, most of the book dealing purely with the stratigraphy. For a treatise of its size, too much space is filled by long lists of fossils, copied from recent palæontological papers on the area. Dr. W. F. Hume will be surprised to see himself referred to on p. 2 as "der inzwischen verstorbene Direktor der Geological Survey of Egypt."

*Exercices de calcul différentiel et intégral.* Par l'Abbé Potron. Premier volume: *Résumé théorique et énonces d'exercices.* Pp. xviii + 332. (Paris: J. Hermann, 1926.) 35 francs.

MOST of the standard French treatises on mathematical analysis expound the subject in a way that gives but little help to a student by supplying him with exercises to test his progress. The volume before us is welcome as a useful supplement to such treatises. It contains more than a thousand unworked examples on the differential and integral calculus, including their geometrical applications, and on differential equations. Many of the problems have been proposed as examination questions in the French universities, and show the tendency of mathematical thought there in recent years. Short and unexhaustive notes on the theoretical groundwork involved are added, but the book is in no sense a treatise on infinitesimal calculus. Although the range covered aims at being a wide one, there are omitted many branches of the subject with which every well-trained mathematical student should be familiar.

W. E. H. B.

*L'Art musical dans des rapports avec la physique.* Par Prof. Jean Becquerel. (Extrait du tome 2 du *Cours de Physique.*) Pp. iv + 79. (Paris: J. Hermann, 1926.) 6 francs.

JEAN BECQUEREL has the unique distinction of being the fourth in a direct line of distinguished physicists extending over more than a century, and occupying, moreover, the same official position in the National Museum, Paris. This portion of the author's "Cours de Physique" is separately reprinted in a handy form useful to both musicians and physicists. In the question of melodic scales it supports Helmholtz as against the strict Pythagoreans.

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# Our Bookshelf.

## Racial Evolution.

*The Pedigree of the Human Race.* By Prof. H. H. Wilder. Pp. xiv + 368. (London, Calcutta and Sydney: George G. Harrap and Co., Ltd., 1926.) 12*s.* 6*d.* net.

In this "Pedigree of the Human Race," the author writes as a professional zoologist "investigating the history of a single animal species which has become universally distributed." His style is one to which readers of books on early man on the eastern side of the Atlantic are little accustomed; it has much of the austerity of a text-book for the schools. There is great gain in clearness by this method of treatment, especially in the early stages, where the author deals far more fully than is usual with the data of palæontology.

As a zoologist the author is inclined to pay less attention to what he regards as the *amour propre* of man. He holds that the differentia of man are insufficient to place him in a separate family. He also puts forward a new system of ethnological classification based upon the view that racial evolution proceeds from an indifferentiated non-specialised stock towards differentiation and specialisation. This method, however, is not new in its conception, but rather in the detailed application in which it has here been worked out. The undifferentiated, least specialised race is, naturally, the Australian, the protomorph, followed by two metamorphic races, Caucasian-Mongolians and Ethiopians. Then come the archimorphs, proto-Caucasian, proto-Mongolian, and proto-Ethiopians. The classification is obviously of great interest, but in detail seems open to criticism. It is necessarily given in skeleton, and before criticising it, it would be desirable to know how the author has handled the data on which it is based. His treatment of the Malay, for example, does not seem entirely satisfactory. "The most singular coincidence in anthropological history" to which the author directs attention, by which Pithecanthropus was brought to light in the year of Haeckel's sixtieth birthday, 1894, he must abandon, no doubt with regret, as that discovery was made in 1891–92, as correctly given on a previous page.

*The Formation of the Greek People.* By Prof. A. Jardé. Translated by M. R. Dobie. (The History of Civilization Series.) Pp. xvi + 359. (London: Kegan Paul and Co., Ltd.; New York: Alfred A. Knopf, 1926.) 16*s.* net.

This volume of the "History of Civilization" Series is an attempt to explain the Greek 'miracle,' meaning, as Mr. Henri Berr says in his preface—not a miracle in the sense of Christian mysticism, but an object worthy of admiration, with an implication of contingency. M. Jardé has given his readers an analysis of the course of events in the various city States in their external relations *inter se* and with other peoples, of their political, social, and intellectual development, of Hellenic expansion and the development of Hellenic unity, which is little short of brilliant. Yet it is difficult to characterise the book as a whole, and gauge its success in achieving its object, for it is a paradox, both as a whole, as the author acknowledges, and also in its parts.

Both anthropologist and geographer may feel that notwithstanding M. Jardé's admirable account of geographical conditions and racial elements, in turning aside to find the answer to his problem in the efforts of man which "turn possibilities into realities," he has perhaps missed the subtler factors of the solution. If to find an explanation in tracing the historical course of events in each State up to the unification of Greece under Macedon be a paradox, it is even more paradoxical to regard the course of events as unconditioned excepting so far as causally connected in succession one to another, when once they have been set in train. After this comment—for it is scarcely a criticism—it is perhaps unnecessary to say that M. Jardé's contribution to the series is both interesting as an analysis of city State history and stimulating, if not provocative, as an exercise in historical method.

*Primitive Culture in Italy.* By Prof. H. J. Rose. Pp. ix + 253. (London: Methuen and Co., Ltd., 1926.) 7*s.* 6*d.* net.

Mr. Rose's companion volume to his "Primitive Culture in Greece," if introduced into schools, might well serve to eliminate much of the dreariness which teachers of the classics have too often succeeded in introducing into the study of Roman religion and social organisation. Postulating a primitive culture and religious belief in which *mana* is the predominant element—*mana*, Mr. Rose points out, is a conception to which *numen* is closely akin—he shows that in classical Italy religion — the gods, worship, and magic — the calendar, the conceptions attaching to priests and kings, and the organisation of the family and the clan, the ideas underlying the law relating to crimes and torts, property, etc., on analysis yield an element referable to certain primitive conceptions; in other words, that much in the observances of classical times is explicable only as survival from prehistoric times. Mr. Rose's treatment of religion is particularly illuminating, and his suggestion that the practical aim of ritual and

observance was essentially occasional and intended to direct *mana* into profitable channels does offer a reasonably sound explanation of the Roman deities, which stand apart from the circle of deities obviously imported from outside.

*Origins of Education among Primitive Peoples: a Comparative Study in Racial Development.* By W. D. Hambly. Pp. xx + 432 + 79 plates. (London: Macmillan and Co., Ltd., 1926.) 25*s.* net.

CONTRARY to popular impression, little boy and girl savages do not live quite a care-free existence, even though they are often more spoiled by their parents than the child of civilisation. The medium of their education often disguises the fact that they are being educated. As with all animals, the early stages of education, largely but not entirely physical, are almost wholly carried on through the play activities—among the boys, mimic hunting, fighting, and care of cattle; among the girls, care of dolls, and simulations of women's work in the field and home. Sometimes instruction may be even more organised and the father will take the boys as his helpers, while the mother requires the assistance of the girls in her work, an appropriate task to be learned at each age. The subject of education among primitive peoples has not been adequately studied, and Mr. Hambly's book serves a useful purpose in gathering together the facts as they have been recorded by travellers and ethnographers. He has brought out very clearly not only the value of the instruction imparted in puberty and initiation ceremonies, but he also emphasises what is apt to be overlooked, namely, the importance of the environmental influence by which the ethical code of the community forcibly, if imperceptibly, impresses itself upon the individual.

## Elementary Botany.

*Elementary Botany: an Introduction to the Study of Plant Life.* By Dr. W. Watson. Pp. viii + 368. (London: Edward Arnold and Co., 1926.) 6*s.* 6*d.*

THE number of elementary botanical text-books published in England and America is now so large that it is perhaps not hypercritical to expect that any addition to their number should contribute originality either in substance or in treatment. The text-book before us departs from the conventional type chiefly in its omissions. Thus, although the reader will find the elaters of the liverwort described both as to their structure and function, the fibrous layer of the angiospermic anther receives but passing mention, and incidentally is erroneously figured. Similarly, though the reader will find details of the microscopic structure of the thallus of Xanthoria, he will not grasp those details of the stoma of the flowering plant upon which its action depends.

The description is simple and direct though at times a trifle colloquial, and not infrequently the phraseology does not sufficiently guard against the perpetuation of common errors. Such a statement, for example, as that "respiration is merely a method whereby oxygen can be supplied to the tissues in the working part of the body and the waste carbon dioxide taken away," will, despite the context, probably lead to grave misconceptions in the student's mind. Again, in the description of the tissue elements, in the explanation of Mendel's results, and elsewhere, there appears to be a lack of appropriate emphasis on essentials.

Suggestions for experimental work of a simple character are interspersed in the text, and the physiological and histological aspects are appropriately interwoven with the morphological treatment.

*A Practical Introduction to the Study of Botany.* (Specially intended for the Use of Indian Students.) By Sir J. Bretland Farmer and Dr. Haraprasad Chaudhuri. New edition. Pp. viii + 300. (Calcutta, Bombay and London: Longmans, Green and Co., Ltd., 1926.) 3 rupees.

IT is some years since the original edition of this book appeared. Since then considerable advances in our knowledge have been made which are not adequately reflected in these pages. It is true that the trend of modern botanical progress is recognised by the inclusion of a chapter on ecology and one on evolution and heredity, but, to employ the jargon of the diplomatist, these amount to little more than 'gestures,' since ecology is disposed of in five pages chiefly devoted to an enumeration of Warming's habitat classes, whilst evolution and heredity furnish the subject matter of barely four pages, of which two deal with the familiar Mendelian segregation.

The chief modifications from the earlier edition concern the substitution where necessary of Indian for European types and appropriate modifications in the families considered. Several errors of the original edition remain uncorrected, as, for example, the figure of the vascular system of the dead-nettle, in which the four main bundles are shown in duplicate, and the figure of *Sonchus crispus* which appears as *Sonchus oleraceus*. We may note, too, the absence of ligules from Fig. 154 purporting to represent a longitudinal section of a Selaginella cone. There are also several irritating if minor misprints, and one cannot but regret that the occasion was not taken to revise the work completely and bring it up-to-date.

*The Story of the Plants.* By Grant Allen. (Hodder and Stoughton's People's Library.) New edition, fully revised and annotated, and with a Biography of the Author by Marcus Woodward. Pp. 200. (London: Hodder and Stoughton, Ltd., n.d.) 2*s.* 6*d.* net.

GRANT ALLEN'S "Story of the Plants" is a work which had a considerable vogue at a time when popular books on botanical subjects were comparatively few. In the present abridged edition, although the original text remains unchanged,

except for deletions, the editor has wisely prefaced each chapter by a short introduction correcting some of the errors of which the author was guilty, and placing the text in relation to modern knowledge. There is, indeed, so much at the present day to modify, either in fact or inference, that the interest of the work is largely historical. Grant Allen took considerable licence as to his manner of expression, whilst some of his statements were not always too well based, but the merits of his writings must be judged by the available knowledge at the date of publication. The time has fortunately long since passed when accuracy of description was incompatible with popular appeal, a position which we owe in no small measure to the activity of natural history societies and the growing recognition that a real understanding of biology is an essential part of a liberal education. Many will therefore welcome the opportunity of becoming acquainted at small cost with a work which played a part in the popularisation of natural science some thirty years ago.

## Plant Products.

*Gum Arabic: with Special Reference to its Production in the Sudan.* By H. S. Blunt. Pp. 47 + 22 plates. (London: Oxford University Press, 1926.) 10*s.* 6*d.* net.

THE author, who is assistant conservator of forests to the Sudan Government, states in the preface that he has been persuaded to place on record some of his observations on gum arabic carried out during five years' work in one of the main gum-producing districts of southern Sudan. A perusal of the book gives the reader quite a vivid impression of the local conditions of collection and cultivation, as well as details as to markets, freight, royalties, water supply, etc., and as a more or less informal record of observations made on the spot it will be read with profit by those interested in the gum-arabic trade. The book cannot, however, be regarded as a serious contribution to the scientific aspects of the question. For example, the opening sentence of the section on chemical properties—"Gum may be regarded as a potassium and calcium salt of gummic or arabic acid of the general formula ($C_6H_{10}O_5$)"—scarcely inspires confidence. Reading on, one finds a detailed account of how to dissolve gum arabic in water for the purpose of determining its viscosity, but there is no indication as to how the measurement is made, and, moreover, the figures given to represent the viscosity appear to show that in five cases out of six the viscosity of a 20 per cent. aqueous solution is less than that of a 10 per cent. solution. Again, a paragraph on p. 43 states that "The author has seen and isolated a bacillus both from a solution made from branches of the tree and from the gum itself. In each case the bacterium seen was exactly the same. The organism appeared to be rod-shaped and was extremely mobile." One is inclined to wonder what a bacteriologist would think of such an identification.

In view, however, of the fact that the author modestly states that he is cognisant of the incompleteness of his work, he would probably lay no claim to writing a scientific treatise, but that is all the more reason for either avoiding scientific aspects or else subjecting the manuscript to critical revision by experts. The numerous illustrations at the end of the book add greatly to the interest of the text, though, strange to say, they are but rarely referred to there.

*Plant Products.* By S. Hoare Collins and George Redington. (Industrial Chemistry Series.) Pp. xiii + 262. (London: Baillière, Tindall and Cox, 1926.) 10*s.* 6*d.* net.

THIS is the second edition of the first author's "Plant Products and Chemical Fertilisers"; the title has now been abbreviated, as the chapters dealing with artificial fertilisers have been slightly curtailed, the subject having been more fully treated in the companion volume, "Chemical Fertilisers." The general arrangement of the book is much the same as before, but the third part has been extended somewhat to include a short account of the main factors operative in the process of photosynthesis, while the fourth part has been modified considerably by the inclusion of modern conceptions of the values of food materials.

Taken as a whole, the book will be found to be most readable and to contain a great variety of interesting information, but in many cases the treatment is distinctly scrappy, and it is to be regretted that some of the facts concerning pectins and mucilages have not been brought up-to-date. The indexing also is very poor; in one case the name Hokkaido of the Japanese University in that place is given as a reference to a paper on acid mineral soils by three authors of entirely different names. It is to be hoped also that the large number of misprints of names of authors, etc., will be corrected in any future edition.

## Horticulture.

(1) *The Principles and Practice of Horticulture.* By A. S. Galt. (Text-Books on Agriculture.) Pp. viii + 240. (London: University Tutorial Press, Ltd., 1926.) 3*s.* 6*d.*

(2) *Fruit Growing.* By Prof. W. H. Chandler. Pp. xv + 777. (London, Bombay and Sydney: Constable and Co., Ltd., n.d.) 21*s.* net.

HORTICULTURE is one of the most ancient industries, yet as a science it is a mere babe. Not a few who are engaged in the industry have yet to awake to the existence of the science. Here are two books which should speed up that awakening.

Though written by teachers well known in the horticultural spheres of England and the United States, these two books are in every other respect as different as, let us say, chicory from cherries. (1) Mr. Galt's book is quite in the traditional style (and English, not American, tradition at that), founded mainly on long-established practice, with all too few references to the results of recent research. Even these few are not always so accurate as might be wished; it is to be hoped that

if a second edition is called for, the table on p. 194, vitally influencing as it should the practice of fruit-planters, may at least be corrected. Again and again one is pulled up short by question-begging statements, based sometimes on observation, often merely on long-cherished opinions. How often these traditional views and practices have had to go by the board is shown in the second of these books.

(2) Prof. Chandler has produced as comprehensive a review of agricultural and horticultural research, in their bearing on fruit-growing, as the most exacting reader could demand. His title, however, is somewhat misleading. Though fruit-growers as a body are far more alive and interested in research than they were twenty, or even ten, years ago, there are still very few in whose hands this book could be placed with some hope of its being adequately studied. That it is directed primarily towards those engaged in research is shown in the introduction, which plunges forthwith into a detailed discussion of the reliability of field experiments, the probable error, and other mathematical concepts used by up-to-date research workers, but meaningless to a very large proportion of practical fruit-growers.

Yet much of this book would be easily assimilable by the average fruit-grower if only he could be persuaded to give his mind to the study of it. The author is far from ignoring current practice, but shows wherever possible to what extent it is justified by the results of research; and he is not afraid to show how frequently the results obtained by different workers are contradictory. He even gives more or less satisfactory explanations of these contradictions, and is always careful to give the reader some indication of the probable reliability of each result. The book is concluded by an admirably arranged and most comprehensive bibliography.

## Farm Management.

(1) *Farm Calculations and Accounts, including a Thorough Treatment of Agricultural Calculations and of the Business Transactions of the Farm.* By Dr. Arthur G. Ruston and C. Vivian Dawe. (Text-Books on Agriculture.) Pp. x + 222. 3*s*. 6*d*.

(2) *Farm Measurements: a Practical Treatment of Problems in Mensuration.* By Dr. Arthur G. Ruston and C. Vivian Dawe. (Text-Books on Agriculture.) Pp. x + 163. 2*s*. 6*d*.

(3) *Agricultural Surveying, including Mensuration, Road Construction and Drainage.* By John Malcolm. (Text-Books for Agricultural Students.) Pp. vii + 313. 5*s*. 6*d*.

(London: University Tutorial Press, Ltd., 1926.)

THE importance of accurate figure work in agriculture, as in costings accounts and measurements of various types, is reflected in the production of text-books specially adapted for the use of farmers and agricultural students.

(1) In "Farm Calculations and Accounts" the preliminary chapters are devoted to a simple exposition of the arithmetical rules, fully illustrated by examples drawn from ordinary farm life. These rules are then applied to such items as household and commercial accounts, valuation and purchase of manures, calculation of costings appertaining to crops, foodstuffs, and livestock. The value of graphs for the diagrammatic representation of facts is indicated, with examples of the various ways in which a graph can be projected. A specially useful feature is a short chapter on the writing of business letters, a matter in which much haziness prevails as to the correct order of procedure.

(2) In their other book referred to above, Dr. Ruston and Mr. Dawe deal with the methods used in obtaining lengths, areas, and volumes associated with farm practice, attention also being given to brickwork and building construction and to matters connected with water supply, work, and power. Measurement of standing timber, spreading of farmyard manure, and estimation of weight of roots per acre are among the special items dealt with in some detail.

(3) Mr. Malcolm's work covers a somewhat different field from the foregoing, in that it includes mensuration, road construction, and drainage. An excellent description is given of methods of field surveying, a point specially to be commended being the mention of many details the knowledge of which makes for smooth and accurate working. The use of the various instruments is well described, and the book should be valuable not only for surveying students connected with agriculture, but also for others.

*Estate Accounts.* By C. S. Orwin and H. W. Kersey. Pp. vi + 45. (Cambridge: At the University Press, 1926.) 3*s*. 6*d*. net.

THE object of estate accounting is to assist in management quite as much as to act as a check to expenditure, but very few books are available which describe desirable methods to be adopted. The present volume aims at presenting a system of estate book-keeping which entails the minimum of clerical labour, and provides the maximum amount of detailed and analysed information. The method suggested is very elastic, and is exemplified by a full year's working of one particular estate, showing all the necessary associated accounts, with the subsidiary books and records in the hands of subordinate workers.

A picture of the financial history of an estate is not easy to visualise from a consideration of the account books for several consecutive years, but it is clearly portrayed by means of graphical representation of the different accounts. Illustrations are given of graphs showing such items as rental, property tax and super tax, tithe rent charges and improvement rent charges, which demonstrate how useful such a method of delineation may be in the hands of estate agents and property owners. It indicates clearly the effect of changes of estate policy, of public policy, and of economic conditions on the fortunes of the estate.

## Oil and Ore Deposits.

*The Location of Mineral Fields: Modern Procedure in the Investigation of Mineral Areas and the Subsequent Verification of their Extent, etc.* By M. H. Haddock. (Lockwood's Manuals.) Pp. vii + 295. (London: Crosby Lockwood and Son, 1926.) 9*s.* 6*d.* net.

THE outstanding merit of this book is that it co-ordinates much useful and practical information scattered through a number of other works. Dispensing with any introductory matter, the author commences to deal with certain geometrical properties of mineral deposits, as dip, strike, depth and thickness of strata, and the determination of these by calculation and by geometrical methods. Two chapters follow on contouring and the form of outcrop and mineral mapping practice. Under borehole surveying, both early and modern methods are discussed; the determination of the dip and strike of a seam by means of three boreholes is taken as a typical practical example and dealt with in all its possibilities. Faulting and folding is treated with the aid of many explanatory figures, and the determination of fault data by calculation is discussed.

The chapter on applied geophysics, a subject on which a separate book is long overdue, deals with the chief physical methods employed for the location of ore bodies, the instruments used, and some applications of these. This chapter is perhaps one of the best available summaries, in English, of the practical details and significance of geophysical methods. It is, however, difficult to see why Germany and Sweden should be persistently regarded as the only countries in which progress in geophysical methods of investigation and prospecting has been made since 1918; important research has been carried out in England, particularly with regard to the Eötvös torsion balance. Chapters on spherical trigonometry and astronomical methods in so far as they concern surveying, and on triangulation, are sound and replete with practical information. The correlation of surface and underground surveys occupies the last chapter.

This book does not concentrate on the theoretical aspects of the problems involved, but attacks each subject from the practical point of view; it is a thoroughly practical disquisition and should commend itself to mining students and others.

E. R. F.

*The Geology of Oil, Oil-Shale and Coal.* By Dr. Murray Stuart. Pp. ix + 104. (London: Mining Publications, Ltd., 1926.) 7*s.* 6*d.* net.

AN ambitious title, a few bold theories, some sweeping deductions based, perhaps, on slender evidence, the forging of a few weapons of attack for the oil geologist's armoury, one or two critical causeries: these constitute the chief features of this unorthodox little volume. Impressed by the fact that oil deposits "retain no fossil evidence of the origin of the oil" (?), the author is led to the conclusion that in such cases it has probably been deposited along with the sediment according to generally accepted principles of deposition; this, the first hypothesis. Turning next to coal, "each little layer . . . represents an individual sedimentary stratum." This implies that coal does not represent a state of carbonisation of vegetable matter *in situ*, but rather the consolidation of black mud directly transported by rivers from freshwater swamps and laid down normally with sedimentary detritus: thus the second theory, originally formulated by Jukes, resuscitated by the late Charles Lapworth in the course of a lecture in 1906, from which the author acknowledges inspiration in his preface.

Petroleum in dolomitised limestones *ipso facto* disposes of the theory of its sedimentary deposition in such cases; hence these special circumstances are examined and the conclusion reached that foraminifera with specific bacterial aid are the influential contributors, fossils of bottom-living forms being absent. A further hypothesis concerns Burma petroleum and suggests the silicified wood of Dipterocarpus as a possible mother-substance, a theory which has recently engaged the critical attention of L. Dudley Stamp. Thus we reach the author's 'six points' for the oil geologist, wherein attention is directed to dolomitised limestones, as above outlined, to foraminiferal or diatomaceous shales minus bottom-living fossils (diatoms have lately been discounted by American geologists as being fundamental in the mother-substance of the really classical example, the Monterey Shale of California), the lateral variation of coal seams through torbanite to oil-shale or into liquid petroleum (given suitable carbon ratios and geological circumstances), the fossil wood origin of petroleum theory, and lastly, the association of oil with dynamically disturbed marine coals.

A penultimate digression discourses on oil in Great Britain (an unhappy subject), future development in Burma, possibilities in north-west India and Australia. The valedictory is, curiously, on "Speculative Geology." But surely the whole book is essentially this? At all events, given a similar frame of mind, the reader will probably not find his time wasted.

## Popular Astronomy.

*Modern Astronomy: its Rise and Progress.* By Dr. Hector Macpherson. Pp. viii + 196. (London: Oxford University Press, 1926.) 6*s.* net.

THIS little book consists of a series of ten lectures delivered by the author as Thomson lecturer in natural science for the session 1925–26 at the Aberdeen United Free Church College. The subject matter of modern descriptive and physical astronomy is treated historically, particular attention being given to those ideas of earlier times from which modern ideas have most directly developed. The period covered by the survey extends from the time of Copernicus up to the present day. Prominence is given to the views, not only of the recognised leaders in the subject, but also of some of those whose conclusions are less widely accepted and have, perhaps, not

received the attention which they deserve. Quotations occur frequently throughout the book.

The treatment is lucid, interesting, and in the main accurate, and the book should prove useful, especially to amateurs with little time for a detailed study of the subject. Those with a slight previous knowledge of astronomical ideas and nomenclature will receive the greatest benefit from its perusal. There are a few mistakes which are not of the first importance, but should be removed in possible future editions. Thus on p. 96 it is stated that Lebedeff verified the *law* of radiation pressure, and on p. 123 Eddington's theoretical researches are said to have *confirmed* the pulsation hypothesis of Cepheid variation. The book is well produced and pleasant to handle, but would have been much improved if the proofs had been read more carefully. There are several misprints which make meaningless the sentences in which they occur, and in one instance at least (p. 110) a false meaning is given when the distance of 61 Cygni is recorded as about forty million (instead of billion) miles. On the whole, however, the book is substantially trustworthy, and can be recommended to those requiring a book of its type.

(1) *The Romance of Comets.* By Mary Proctor. Pp. xiii + 210 + 16 plates. (New York and London: Harper and Bros., 1926.) 7*s.* 6*d.* net.
(2) *Legends of the Sun and Moon.* By Mary Proctor. Pp. 159. (London, Calcutta and Sydney: George G. Harrap and Co., Ltd., 1926.) 1*s.* 3*d.*

(1) "The Romance of Comets" is essentially a book for leisure reading, and is written primarily for those possessing little or no knowledge of elementary astronomy. Throughout, considerable space is devoted to anecdotes and reminiscences, most of which contribute materially to the readableness of the book, although one or two are interpolated rather unnecessarily. The titles of the nine chapters indicate the scope of the contents, as, for example, (1) comets as portents, (4) comets in distress, (6) return of Halley's Comet in 1910, (8) meteor streams. Chap. ii., "Comet-Hunting as a Hobby," which contains a delightful account by the late Prof. Barnard of "The house that was built with comets," should win more than one reader to the ranks of amateur astronomers.

In spite of the use of simple language and several helpful analogies, some passages are obscure or convey a wrong meaning (pp. 15, 83-84, 95, 121). The introduction of "Hints for Amateur Photographers," pp. 83-91, is somewhat pointless, as the reader on perusal will be led to infer that the equipment required for cometary photography lies far beyond his means. A few misprints have been passed, the most important being in the footnote on p. 125—*for* 1965 *read* 1985. The type is pleasant to read, and the illustrations are good. On the whole, it may be said that this book will be read with pleasure and interest by those who wish for a simple account of these remarkable objects to which astronomers have devoted so much study and patient observation since the time of Edmund Halley.

(2) In "Legends of the Sun and Moon" Miss Proctor has produced for young readers an interesting collection of myths, superstitions, and customs relating chiefly to the sun and moon.

## Relativity.

(1) *Readable Relativity: a Book for Non-Specialists.* By Clement V. Durell. Pp. vii + 146. (London: G. Bell and Sons, Ltd., 1926.) 3*s.* 6*d.* net.
(2) *From Kant to Einstein.* By Hervey de Montmorency. Pp. iii + 39. (Cambridge: W. Heffer and Sons, Ltd.; London: Simpkin, Marshall and Co., Ltd., 1926.) 2*s.* 6*d.* net.

(1) THE title of Mr. Durell's book is somewhat misleading. The book is readable only to those whose familiarity with algebra approaching matriculation standard is very intimate, for it abounds in simple mathematical formulæ. However, for such inquirers it is certainly very readable, and is one of the best expositions of its kind that we have seen. The fundamental principles of the theory are very clearly explained, and almost every general statement is illustrated by a good numerical example, for which many readers will be very grateful. Each chapter concludes with a number of test questions, of which the answers are given at the end of the book. The treatment in parts is perhaps too detailed; thus the deduction of the Fitzgerald-Lorentz equations of transformation occupies eleven pages, although mathematical symbols are used and all the necessary preliminary ground has previously been covered. This tends to obscure rather than to illuminate.

We must dissent from Mr. Durell's statements that the progress of modern astronomy "is due entirely to the successive aids that inventions have given in supplementing the powers of naked eyes and naked hands" (p. 8), and that whenever the separation between two events is real, "there is some causal connection between the events" (p. 77). On the whole, however, the book has given us great pleasure, and can be confidently recommended as a trustworthy and interesting guide to Einstein's theory. This applies particularly to the account of the restricted theory of relativity, to which about five-sixths of the book is devoted.

(2) Mr. de Montmorency's book appears to be a not wholly intelligible attempt to judge Einstein's theory of relativity by the standard of Kant's philosophy. As he puts it: "Kant's doctrines may serve as a canon for judging Einstein's propositions." The chief point seems to be that time cannot be "subordinate to space," as Minkowski and Einstein are said to make it. Mr. de Montmorency insists upon the fact that Einstein is not a metaphysician (without, however, apparently realising that his theory is a scientific and not a metaphysical one); he states that if he were, "he would choose his language with more care," and asserts that "many of Einstein's arguments are open to criticism and contain internal evidence of carelessness." These remarks, however, fail to carry much weight when their author tells us that "Newton accounts for the orbit of a planet

thus: its path is the resultant of two forces; (1) the force from which its initial velocity was derived; (2) the force of gravity pulling it towards the sun." When, a little later, we read that plane Cartesian equations "are always in two unknown quantities and of the second degree," and that by introducing a third co-ordinate we can form the equation to a spiral which "will be in three unknown quantities and of the third degree," our respect for his criticism dwindles to vanishing point. The book concludes with an appendix consisting of "a few Lorentz-Einstein equations of transformation." H. D.

## Laboratory Physics.

*High Vacua.* By Dr. G. W. C. Kaye. Pp. xii + 175 + 4 plates. (London: Longmans, Green and Co., Ltd., 1927.) 10*s.* 6*d.* net.

VACUUM technique is becoming as popular a subject as relativity, but it offers less scope for originality of treatment. Dr. Kaye's book, containing his Cantor Lectures, is later than the others, and therefore contains a few things that his predecessors omit; it is shorter, and therefore omits many things that they contain. It seems to be equally accurate; the section on vacuum pumps—not in our opinion the most important aspect of the subject—is especially complete. The question whether it meets any demand not yet adequately met is one for the publishers rather than for the reviewer.

*Notes on Practical Physics: for Junior Students.* By Prof. C. G. Barkla and Dr. G. A. Carse. Second edition. Pp. xii + 119. (London and Edinburgh: Gurney and Jackson, 1926.) 6*s.* 6*d.* net.

THE second edition of the "Notes on Practical Physics" used by junior students in the University of Edinburgh does not differ greatly from the first. Some experiments have been added in the section on electricity, and corrections have been made. There is an interesting section on treatment of observations and determination of error—a somewhat unusual feature in a book of this standard, but one which is to be welcomed. The tables of physical constants at the end of the volume might well be revised and brought up-to-date.

*General Physics for the Laboratory.* By Prof. Lloyd W. Taylor, William W. Watson and Prof. Carl E. Howe. Pp. vi + 247. (Boston, New York and London: Ginn and Co., 1926.) 2.40 dollars.

THE feature of this attractive volume which first compels the reader's attention is the excellent series of half-tone illustrations from photographs, showing the apparatus set up, ready for use, including all minor auxiliary instruments. The authors point out that this plan does away with the necessity for a detailed description of the apparatus.

The book forms a text-book of practical physics containing nearly fifty experiments covering a course such as might be followed by a student working for a pass degree in Great Britain. An account of the theory of each experiment is given, and the laboratory instructions are formulated in an unusually specific and detailed way. This method is defended by the authors, who point out that even instructions such as these are not too easily followed by a student entering on an unfamiliar field of manipulation. One question, however, suggests itself: Does the carefully drilled student turned out from a modern highly equipped and carefully organised laboratory possess the initiative and resource of his less fortunate predecessor, who was 'thrown in at the deep end'? Granting the position assumed by the authors, the book is excellent and well planned, containing many novel and ingenious experiments. Special mention may be made of an experiment for finding the acceleration due to gravity by means of a freely falling weight, the position of which is registered on a strip of paper by electric sparks jumping at regular intervals.

The statement on page 211 that the Nicol prism was invented by "the German physicist Nicol" requires correction. William Nicol of Edinburgh, who describes himself as a lecturer on natural philosophy, published a description of his polarising prism in the *Edinburgh New Philosophical Journal* in 1829.

## General Inorganic Chemistry.

*General Inorganic Chemistry.* By Prof. M. Cannon Sneed. Pp. vi + 674. (Boston, New York and London: Ginn and Co., 1926.) 12*s.* net.

THE author has made an excellent attempt to "reach a proper balance between descriptive and theoretical matter" in this work, and although undue prominence is given to the work of the American Chemical Warfare Service, the book provides a good introduction to the subject. The description of the commoner elements, though not overloaded with detail, deals with essential points, and the simple diagrams, especially those of industrial processes, are admirable. Brief accounts are also given of many of the rarer elements. This descriptive work is relieved at intervals by short historical notes, with excellent full-page portraits of outstanding personalities, and by chapters on such topics as electrolytic dissociation, the structure of the atom, thermochemistry, ionic equilibria, colloids, chemistry in living processes, radioactivity, etc. The beginner may be confused by the use of the two units of volume, the cubic centimetre and the millilitre, and by the inaccurate *definition* on p. 125 of atomic weight as the least weight of the element in a molar volume. Nor will he easily grasp the difference between an ordinary current of positive electricity and the current of electrons on p. 642, but on the whole the style is clear.

No misprints have been noted, but there are a few grammatical faults, *e.g.* 'there are a great variety of them' (p. 480). Such words as 'typal,' 'inhomogeneous,' and 'reactant' have an unfamiliar sound. Aluminium and niton have been re-named aluminum and radon, whilst elements

Nos. 43, 61, and 75 are boldly called masurium, illinium, and rhenium respectively, and a formula is given for the still unknown carbonate of aluminium.

*A Text-Book of Inorganic Chemistry.* Edited by Dr. J. Newton Friend. (Griffin's Scientific Text-Books.) Vol. 3, Part 2: *Beryllium and its Congeners.* By Joshua C. Gregory and Dr. May Sybil Burr (née Leslie). Pp. xxvi + 342. 18*s.* net. Vol. 7, Part 3: *Chromium and its Congeners.* By Reece H. Vallance and Arthur A. Eldridge. Pp. xxvi + 380. 18*s.* net. (London: Charles Griffin and Co., Ltd., 1926.)

THE two new sections of Dr. Friend's "Text-Book of Inorganic Chemistry" deal respectively with (i) beryllium, magnesium, zinc, cadmium, mercury, and (ii) chromium, molybdenum, tungsten, and uranium. They conform so closely to the pattern already established by earlier volumes of the series that very little special comment is called for. It may, however, be said that the type to which the successive volumes conform with increasing definiteness is that of a work of reference, which contains an array of facts which makes it almost impossible for the casual reader to discover any sections which can be read consecutively with any degree of enjoyment. For this reason, indeed, it appears likely that many of those who appreciate at their full value the merits of the "Text-book" will put the new volumes in their appropriate places on the shelf without attempting to 'read' them, but with the definite object of referring to them when a suitable opportunity arises. It may perhaps be regretted that the general editor of the series has discouraged his contributors from 'letting themselves go,' since the whole work might have been transformed by the introduction of a number of chapters in which the individuality of the authors was allowed to stand out, especially when dealing with the more exhilarating topics.

*A Systematic Qualitative Chemical Analysis: a Theoretical and Practical Study of Analytical Reactions of the more common Ions of Inorganic Substances.* By Prof. Geo. W. Sears. Second edition, revised. Pp. x + 165. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1926.) 10*s.* net.

THIS book commences with a brief account of the ionic theory of solution together with the laws of mass action and solubility products, and continues with the systematic analysis of cations and, finally, anions. The subject matter contains no new methods of analysis, but is based on the methods of Treadwell and similar standard works. The first section, dealing with mass action and solubility products, has been well written and is easily understood. The later sections, however, are in places somewhat confusing to the beginner. Statements such as "treat the filtrate by (10) and the residue by (2 c.)," or "treat the filtrate by (20). If there is a residue . . . treat by (7)," which are very common throughout the book, do not make the scheme of analysis easy to follow. The book does not seem to have any advantage either in price or subject matter over the numerous other works on this subject.

## Organic Chemistry.

*A Student's Manual of Organic Chemical Analysis: Qualitative and Quantitative.* By Prof. J. F. Thorpe and Prof. M. A. Whiteley. Reissue with Appendix on New Methods of Organic Analysis, by H. ter Meulen and J. Heslinga. Pp. x + 250. (London: Longmans, Green and Co., Ltd., 1926.) 9*s.* net.

IT is an indication of the value of this work that a reprint has been required so soon. The new issue differs from the original volume reviewed in NATURE, Nov. 14, 1925, p. 707, in one particular only. At the end of the book there is added an appendix of 41 pages by H. ter Meulen and J. Heslinga on improved methods of ultimate analysis. These authors find that, if manganese dioxide is used instead of copper oxide, a lower temperature of combustion can be employed with resultant economy in tubes, etc. They describe a method whereby oxygen can be estimated by hydrogenation and absorption as water, allowance being made for unchanged carbon dioxide which is absorbed by soda lime. Nitrogen is estimated either by hydrogenation, using powdered nickel at 250° as a catalyst and titration of the ammonia produced, or by combustion with manganese dioxide and collection as nitrogen. Similarly, sulphur and the halogens are estimated by hydrogenation to the volatile acids. From the particulars given, several of the methods seem a considerable advance on those usually employed. The reviewer again recommends this book to all students.

*Theoretical Organic Chemistry.* By Dr. Francis Arnall and Francis W. Hodges. Part 1. Pp. xi + 372. (London: J. and A. Churchill, 1926.) 10*s.* 6*d.* net.

THIS book has been written on the usual lines of organic chemistry text-books, but suffers from a desire to include too much information in too small a space. An endeavour is made to teach organic chemistry from the beginning to pass degree standard in 360 pages. The result is that a certain amount of explanatory matter has been omitted and the student will probably fail to obtain a clear and intimate knowledge of the subject without further assistance. For students preparing for examinations it is, however, eminently suitable, since it is remarkably extensive in scope, contains nothing unessential, and is quite up-to-date. Not only are the principal types of organic compounds described with the reactions, identification, and commercial application of their simpler representatives, but also there are chapters on synthesis, isomerism, tautomerism, stereoisomerism, ultimate analysis, molecular weight determination and estimation of typical groups. We have noticed that while a description of synthol is given, no

mention is made of the more important commercial synthesis of methyl alcohol. The description of methylated spirits (p. 53) is inaccurate. Other errors noted are the formulæ of nitrolime (p. 98), phenyl isocyanide dichloride (p. 103), citric acid (p. 158), anthranilic acid (p. 318), while Liebermann is misspelt on p. 121.

*Dyestuffs and Coal-Tar Products: their Chemistry, Manufacture and Application.* By Thomas Beacall, Dr. F. Challenger, Dr. Geoffrey Martin and Dr. Henry J. S. Sand. (Based on Chapters appearing in "Industrial and Manufacturing Chemistry: Organic.") Fourth edition revised. (Manuals of Chemical Technology, 1.) Pp. xi + 168. (London: Crosby Lockwood and Son, 1926.) 16*s.* net.

THIS book forms one of a series of manuals of chemical technology compiled with the object of giving concise but sufficient information concerning the manufacture and utilisation of chemical products of great industrial importance. The first edition was produced during the critical year of 1915, when "the lack of an English book on the subject, the sudden stoppage of the supply of German fine chemicals, and the preparations of the British Government for the establishment on a large scale of the synthetic dye industry in this country, all combined to provide opportunity and justification for the appearance of the book." The authors were not disappointed in their object; the book met with a ready appreciation, and has now reached its fourth edition.

Within the limits of 156 pages a mass of valuable and accurate information is comprised dealing with: (1) coal tar and coal-tar products; (2) the synthetic colouring matters; (3) natural dyestuffs; (4) the dyeing and colour-printing industry; (5) modern inks; (6) saccharin and other sweetening chemicals; (7) modern synthetic drugs; and (8) photographic chemicals. Each of the eight chapters is prefaced by a list of the titles and authors of standard works on the subject dealt with therein, and contains a short historical or descriptive introduction, and usually also some statistical data.

In the preface to this edition the authors refer to the strenuous efforts that have been made since 1915 in Great Britain and other industrial countries to establish dyestuffs and allied industries on a scale commensurate with national needs; and they express the hope that this new position is reflected so far as is possible within the modest limits of their book. A critical examination of the new edition from this point of view is on the whole disappointing; many of the manufacturing processes described have long been obsolete, as, for example, the preparation of ketones (p. 22), of saccharin (p. 125), and of indigo (p. 51); and, further, no statistical data later than 1913 are included. But this book is evidently not written for the critical reader, otherwise the fairly numerous errors in nomenclature and formulæ that crept into the first edition could not have escaped correction in the fourth. M. A. W.

## Clinical Laboratory Methods.

*A Manual of Clinical Laboratory Procedure: for the Use of the General Practitioner.* By Dr. Robert A. Kilduffe. Pp. 287. (London: Henry Kimpton, 1926.) 12*s.* 6*d.* net.

IT is regrettable that the value of clinical laboratory investigations is not realised by many physicians; anæmia is sometimes treated for months without a blood-count having been carried out; dyspeptics are rarely given a test-meal before necessity compels them to be referred to hospital or consultant, and many cases of nervous and mental disorder reach a comparatively advanced stage before lumbar puncture is performed. It is easy to find the reasons for this neglect. The average general practitioner has little time to spare for the laboratory, and expense frequently prohibits consultation with a pathologist. There are, however, many laboratory tests which can easily and rapidly be carried out by the practitioner himself. The object of Dr. Kilduffe's book is to indicate those which are likely to be of value and to assist in the interpretation of their results. There could not be a better manual for the physician who is willing to devote a little time to laboratory work.

Of particular interest are the chapters on the equipment of the laboratory and home-made laboratory devices, though the limitation of the use of such an instrument as the home-made colorimeter must be borne in mind. The arrangement of the book is that usually adopted for laboratory manuals; there are chapters on urine, blood, gastric contents, cerebro-spinal fluid, etc., in which the appropriate tests are described. Few individual methods are given under each heading, the object being to indicate only those most likely to be of value to the practitioner. The discussion of the results and their interpretation is valuable, but more stress might have been laid on the association of the presence of lactic acid with achlorhydria in the diagnosis of gastric cancer. The indexing is not very complete, but a list of common diseases and clinical conditions with suggested laboratory investigations will be a useful guide to the physician.

*Laboratory Outlines in Bacteriology and Immunology.* By Prof. John F. Norton and Prof. I. S. Falk. Pp. viii + 114. (Chicago, Ill.: University of Chicago Press; London: Cambridge University Press, 1926.) 10*s.* net.

THE matter of this small book is in strict accord with its title; it is a programme of work for a course of instruction in bacteriology, serology, and immunology. Few details of actual work are given, and the book is essentially one for the teacher and may be a helpful guide to him, though of course any teacher worthy the name will arrange his own courses of instruction. The sections seem to be well devised and to cover most of the problems of pathological and medical bacteriology, but agricultural bacteriology is scarcely touched. The contents conform with American practice; thus, we find no mention of "carbolic acid coefficients"

under disinfection, or of the "Dreyer method" under agglutination. Directions are given for the preparation of the dye for Leishman's stain, which is now stocked by all dealers, and the impression is given that students are unrestricted in the performance of experiments on animals. A book of this kind must necessarily have a limited circulation, hence perhaps its high cost. R. T. H.

## Technical Electricity.

*Electric Circuit Theory and the Operational Calculus.* By John R. Carson. Pp. ix + 197. (New York: McGraw-Hill Book Co., Inc.; London: McGraw-Hill Publishing Co., Ltd., 1926.) 15*s.* net.

THIS book is the outcome of a course of lectures given by the author in 1925 to the University of Pennsylvania. After a brief introduction to electric circuit theory, a critical exposition is given of the Heaviside operational calculus. It is pointed out that Heaviside's method is known to and employed by only a few specialists. This is partly due to the intrinsic difficulties of the subject and to obscurities in Heaviside's own exposition. In the present work the Heaviside rules are deduced from an integral equation.

The second part of the book deals with the propagation of current and voltage in transmission lines and wave filters. The simplest of the transmission systems is the non-inductive cable, the theory of which was given in Kelvin's early work on the possibility of trans-Atlantic telegraphy. In particular, the physical phenomena can be studied best, as the author shows, when we consider an infinitely long cable. We are sorry that the skin effect in cylindrical cables and the eddy current losses in them are not dealt with, as they give such an excellent illustration of Heaviside's methods.

The author considers that Heaviside overestimated the value of power series in solving problems. In particular he objects to Heaviside criticising those mathematicians who prefer solutions in the form of definite integrals. It is possible that power-series solutions have only a restricted utility in some cases and that definite integrals have proved useful when suitable tables are available. We think, nevertheless, that Heaviside's criticism was justified. Poisson, for example, expressed the capacity coefficients of two spheres as definite integrals, and Dr. Barnes, now Bishop of Birmingham, expressed them as double gamma functions, but when engineers want their values they use series formulæ or interpolate from Kelvin's tables. The book can be recommended to mathematical physicists.

*Alternating Current Rectification and Allied Problems: a Mathematical and Practical Treatment from the Engineering View-Point.* By L. B. W. Jolley. Second edition, revised and enlarged. Pp. xxii + 472 + 27 plates. (London: Chapman and Hall, Ltd., 1926.) 30*s.* net.

WE were pleased to see that a second edition of this book has now been published. Whilst alternating current seems to be almost universally used in large power stations, there is a considerable demand for direct current for auxiliary and control circuits. To obtain this current it is now usual to employ some form of rectifying apparatus. The rectified current is generally pulsating, and it is necessary to know the magnitude of the ripple in it. The author, therefore, starts by giving an account of Fourier's theorem and the methods employed in practice to find the harmonics. From the engineer's point of view this chapter will be found very satisfactory.

Recent improvements in the design and manufacture of all kinds of rectifiers have been noted in this edition. There are also three new chapters on the installation of thermionic rectifiers, on radio supplies, and on inverters. It is pointed out that while the mercury rectifier is essentially a heavy current low-voltage device, the thermionic rectifier is a high-voltage device, and can only be efficiently used when the rectified voltage is of the order of thousands of volts.

The chapter on radio supplies will be of interest to all broadcast receivers. When the house is supplied by alternating current, a rectifier can be used to charge the batteries generally used with a receiving set. In most cases the interest per annum on the capital outlay of a rectifier is less than the cost per annum of charging the batteries by sending them to a local contractor. Where direct current supply is available, the charging can be easily done, but care must be taken not to earth the companies' mains. This can be provided against by inserting a two-microfarad condenser between the radio set and the earth.

*Elements of Alternating Currents and Alternating-current Apparatus.* By Prof. J. L. Beaver. Pp. 370. (New York and London: Longmans, Green and Co., Ltd., 1926.) 18*s.* net.

THIS book proves that alternating-current theory is settling down into orthodox lines. It is written partly to help the average student and partly to help the more ambitious student who is anxious to master the complete theory. Numerous examples are given at the end of many of the chapters which the ordinary student will not have much difficulty in doing. There is a 'snap' about some of these questions which we miss in Great Britain. For example, p. 67: "Prove that $L\frac{di}{dt} = \ldots = \text{what}?$" p. 213: "The exciting current is what per cent. of the name-plate current?"

The capacitance (capacity) of a conductor is defined, but the capacitance of a condenser is not. We do not agree with the author when he says that effective current, effective voltage, and average power are not ordinary algebraical quantities and cannot be handled by ordinary algebraical methods. We think that they are perfectly real numbers. If we want to combine alternating currents and voltages, we have to use the parallelogram law, but the effective values of the components and their resultants are real numbers. The author has been successful in simplifying considerably the theory of polyphase currents and machines. If we neglect

the 'losses' in a machine we get, as a rule, a clear idea of the principle of its working, and we can then gradually build up a more accurate theory. This method is to be commended.

## Use of Electric Power.

*Electric Trains.* By R. E. Dickinson. Pp. xii + 292. (London: Edward Arnold and Co., 1927.) 16*s.* net.

THERE are few industries in such a rapid state of development as electric traction, and there are few in which the literature of the subject is, comparatively speaking, so small. Some of these books are quite out-of-date; others are too advanced, demanding a knowledge of mathematics that few possess; and others contain little beyond a great mass of statistics and data which, while useful to the specialist, are of little help to students and ordinary railway engineers. This book is specially written for the latter two classes and will prove helpful.

To meet the ever-growing competition of tramway and motor-bus traffic, it is necessary to speed up the trains on suburban lines. With steam-hauled trains the acceleration is rarely so great as "half a mile per hour" per second, whereas an acceleration of 1·5 m.p.h. per second can easily be obtained by electric traction. Slipping of the wheels takes place much more readily with steam-operated trains, owing to the pulsating nature of the tractive force. There is also much less wear of the rails when the trains are operated electrically.

The author's chapter on the mechanics of train movement can be readily understood by any one having an elementary knowledge of mechanics. The control systems and the methods of making rheostatic calculations are clearly described. Train resistance curves are given, so that even although this resistance varies considerably with local conditions, useful approximations can be made. It will be remembered that the Advisory Committee appointed by the British Ministry of Transport reported in favour of standardising the supply pressure at 1500 volts direct current. From the data given, it appears that in practice modern railway engineers prefer this system to the single-phase system.

*The Use of Power in Colliery Working: a Treatise on Mining Costs and Machinery Designs and Management.* By John Kersopp. Pp. xxviii + 580. (London: H. F. and G. Witherby, 1926.) 40*s.* net.

DURING the past twenty-five years, mining engineering has been revolutionised by the substitution of power for hand labour in the majority of the operations involved in the winning and working of collieries, and we seem to be within sight of the time when the pick and the shovel will have a place in the museum only. It is perhaps inevitable that the advent of machinery should have led to a slackening of physical effort on the part of the manual worker, so that the net result is that the output per man-shift has remained stationary. Some idea of the variety of machinery at the command of the miner is given by the book under review, but it is by no means an exhaustive treatment of the subject. For example, little or no reference is made to compressed air (an important omission) or to ventilating machinery. Nor is any attempt made to describe banking out or screening arrangements at the surface, amongst which certain modern devices such as decking machinery are of great interest at the present time.

Little attempt is made to explain the mechanics of the machinery described. The chapter on winding engines would have had more value had moment diagrams been given for the principal types of winding engines, and the chapters on pumping would have been similarly improved had the characteristic curves of the different types of pumps been illustrated. In the descriptions of electrical plant there is no reference to the synchronous induction motor, or to the cascade, or A.C. commutator motor. The book indeed appears to be largely a collection of catalogue descriptions and illustrations of a medley of types of apparatus. It contains a considerable mass of useful information badly digested. The use of many photographs showing external details only, and the lack of diagrams illustrating important features, militates against its usefulness. Moreover, the invidious practice of singling out the names of certain firms for special mention is to be deprecated.

At the end is given supplementary information on the various chapters, but it is difficult to understand why these were not included in the chapters themselves. Appendices giving useful information as to fluctuations in costs of materials, wages, etc., are given, but their connexion with the subject matter of the book is not clear. The book contains useful information but will have a limited utility. DOUGLAS HAY.

## Miscellany.

*Organised Publication: a Connected Series of Proposals relating to the Publication and Record of Scientific and Technical Information.* By J. F. Pownall. Pp. 91. (London: Elliot Stock, 1926.) 5*s.* net.

MR. POWNALL'S work is a plea for the international standardisation of scientific literature in respect of page size, spacing of perforations of sheet or leaf, the definition of subject headings in terms of an international code, and the prefixing of the standardised subject headings to each article. Scientific literature, in short, is to be built up from standardised units to be circulated temporarily in volume form, but ultimately to be resolved into unit form and to be housed in temporary binders.

Within recent years some progress has been made in the direction of the standardisation of the pages of the higher class of scientific periodicals, both as regards the quarto and octavo sizes. As the movement appears to have slowed down, it is probable that scientific workers are content with the progress realised and that present methods of publishing the *Proceedings*, etc., of the learned societies are sufficient for economical filing. It is, of course, difficult to say precisely how far size standardisation should be pushed; but it is

tolerably clear that works of the same bibliographical type and dealing with the same or cognate subject matter should be issued in substantially the same size and style. The Patent Offices of the world have long accepted the imp. 8vo size for their specifications—with great benefit to their users; but trade catalogues continue to be issued in oblong folio and other strange sizes which defy classification and shelf arrangement. The trend of book classification is, no doubt, toward uniformity; but this is due, not to any great or general enthusiasm for uniformity on the part of librarians, but to their appreciation of the fact that advanced schemes of book classification—and nowadays elementary schemes are retained only in the smaller libraries—must emanate from libraries which possess the requisite material, training, and financial support. It is, however, doubtful whether these advanced schemes of classification are suitable for the analytical treatment of the contents of periodicals.

Mr. Pownall has, we think, concentrated his attention too closely upon the mechanics of classification. His proposals go far beyond the merits of the case which undoubtedly exists for further standardisation. Publishers have to consider the wishes and prejudices of their clients. No book-lover would accept a book which contained, in addition to the wounds inflicted by machine sewing, the standardised perforations of Mr. Pownall.

*The Radcliffe Infirmary.* By Alexander George Gibson. Pp. xi+316. (London: Oxford University Press, 1926.) 18*s.* net.

Dr. A. G. Gibson, who for many years has been attached to the staff of the Radcliffe Infirmary, Oxford, has given us in the present volume a lively and richly documented account of the history, management, staff, and activities of this institution. Founded in 1759, but not opened to patients until more than ten years later, the Radcliffe Infirmary has always been one of the leading provincial hospitals in England, and, owing to its association with the University of Oxford, occupies a prominent position in medical education. From the very first it was recognised that the Infirmary was to provide opportunities for the study of disease for the future medical graduate. In 1913 the teaching of pathology as a university subject was inaugurated by Sir John Burdon Sanderson, with the result that considerable improvement took place in the mortuary accommodation at the Infirmary. Since the War the whole of the medical staff of the Infirmary has contributed towards university teaching, including the Regius professor of medicine, who occupies the post of consulting physician.

To many the most attractive part of the work will be the biographies of the various members of the medical staff, which numbered among others John Kidd, James Ogle, Charles Daubeny, William Greenhill, Sir Henry Acland, George Rolleston, and Sir William Osler. From the concluding chapter we learn that within the last forty years several new departments have been added to the Infirmary, such as the Dental Department in 1886, the Dermatological and Ear, Nose, and Throat Departments in 1906, the X-ray Department in 1907, the Pathological Department in 1913, and the Orthopædic and Neurological Departments in 1918. The appendix contains, among other items, lists of legacies from 1761 to the present time, preachers of the Radcliffe Infirmary sermon from 1771 to 1862, and members of the committee of management from 1848 to 1921.

*Shipbuilding and the Shipbuilding Industry.* By J. Mitchell. (Pitman's Common Commodities and Industries Series.) Pp. xi+116. (London: Sir Isaac Pitman and Sons, Ltd., n.d.) 3*s.* net.

For the student of naval architecture and shipbuilding there are many text-books. With these this little volume is not intended to compete. It is written almost entirely for the layman who wishes to gain an insight into the position of the shipbuilding industry, the organisation of a shipyard, and the work involved in the designing, building, and fitting-out of ships. In many industries there are technical matters of general interest, and here the reader will find clear explanations of some of the methods of the drawing office and building slip.

The author is himself a shipyard manager and also a teacher, and the headings of some of the chapters indicate his method of treatment. After a brief review of shipbuilding statistics, he deals in turn with types of ships, the shipyard, the designing and the building, launching, and maintenance of ships. There are few more anxious moments than those prior to a launch. A mishap may be disastrous, as was the case of the *Independencia*, launched on the Thames in 1874. Mackrow, who launched so many fine ships at Blackwall, used to say that he never thought of a launch without the *Independencia* appearing like a phantom before his eyes. As Mr. Mitchell says, launching a ship is not a 'pretty-pretty' spectacle got up as an episode in its career, but "the real matter is the serious problem of shifting a weight of some thousands of tons through several hundred feet by means of innumerable pieces of wood and a few hundredweights of soft soap and tallow." His description of the methods employed is one of the most interesting things in the book.

*The Book of the Aeroplane.* By Capt. J. Laurence Pritchard. Pp. 255+24 plates. (London: Longmans, Green and Co., Ltd., 1926.) 7*s.* 6*d.* net.

Captain Pritchard's book is written with the advantage of a good deal of personal experience. It does not profess to deal in any detail with pre-War flying, but gives a very full account of the present position of heavier-than-air machines. Whether these will eventually be called 'airplanes' rather than 'aeroplanes' remains to be seen, though there is a tendency to substitute the former term in some quarters. The author makes an interesting forecast of future developments, which will, in his opinion, be eventually much more pacific than warlike. The book is profusely illustrated and contains a useful glossary.

*Printed in Great Britain by* R. & R. Clark, Limited, *Edinburgh.*

Supplement to NATURE

No. 2995 MARCH 26, 1927

## The Bicentenary of Newton's Death.[1]

By The Lord Bishop of Birmingham, Dr. E. W. BARNES, F.R.S.

WE meet to-day to commemorate by a religious service the bicentenary of the death of Sir Isaac Newton. It is quite characteristic of the attitude of the English people towards the national Church that a group of mathematicians and astronomers, laymen all, should have desired to have such a religious service as the conclusion of their celebrations. They know, as every educated man knows, that Newton's splendid achievements, enshrined in his "Principia," finally overthrew certain beliefs which at the time of the Reformation were held by all, Catholics and Protestants alike. They know that the contest in which he won the decisive victory was but the first of a number—I mention geology, Darwin and Biblical criticism—in which scientific method has successfully challenged views associated with traditional theology. None the less, they expect the English Church to be liberal, to join unreservedly in praising the genius of a great Englishman and in thanking God for his life-work. Their expectations are not disappointed, for the English Church is as illogical and, at its best, as sensible as the average Englishman.

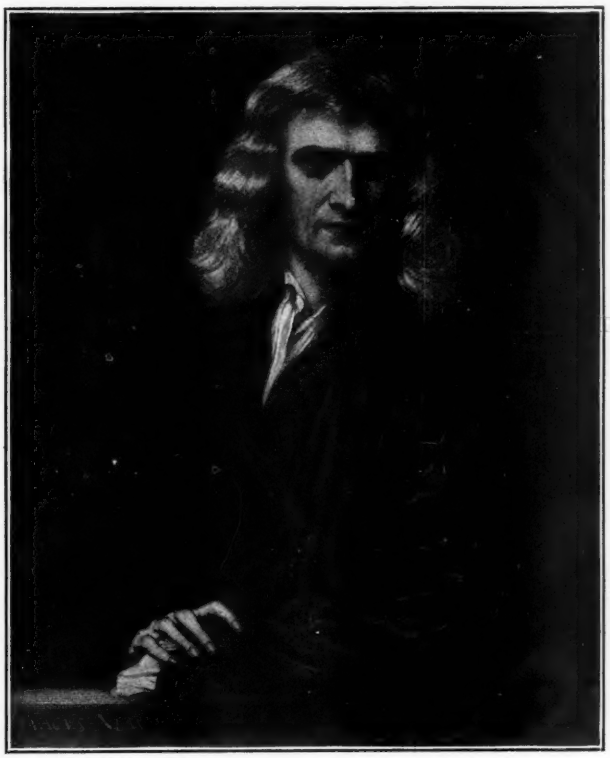


FIG. 1.—Portrait by Kneller, 1689.

From an engraving by Barlow, in the possession of the National Portrait Gallery.

To foreigners we are always something of a puzzle. The Thirty-nine Articles of our Church, to which every clergyman still subscribes, belong to the pre-Copernican period of knowledge. They were in substance drawn up only ten years after a printed copy of Copernicus's great treatise was put into his hands on his death-bed in 1543. Our Prayer Book was last revised in 1662, a few years before the ideas of the "Principia" first took shape in Newton's mind. The Church has thus apparently ignored the vast change in human thought which science has made since the Renaissance. Its theology seems to be associated with crude beliefs as to the history and structure of the universe which were held in antiquity. But, in fact, its theology has been continuously re-shaped by its leading divines, and the process has not yet ended. So Newton lies under a monument in Westminster Abbey, and Darwin was buried near-by in the same great church. "Doubtless in each case such honour was rendered by order of the Government," a foreigner would conjecture. No: English clergymen still have the power to say who shall be buried in Westminster Abbey.

It is then with no misgivings, with no hesitation, that to-day we meet to praise Newton in the parish

[1] A sermon delivered in Grantham Parish Church on Sunday, Mar. 20, in connexion with the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton arranged by the Yorkshire Branch of the Mathematical Association. Other addresses will be found in succeeding pages.

church of the town where he went to school. Six miles away he was born, on Christmas Day, 1642, a puny infant so small that they might have put him into a quart mug. In this neighbourhood he lived for the first eighteen years of his life, an inventive, meditative boy obviously unsuited to farm the small manor which his father had owned.

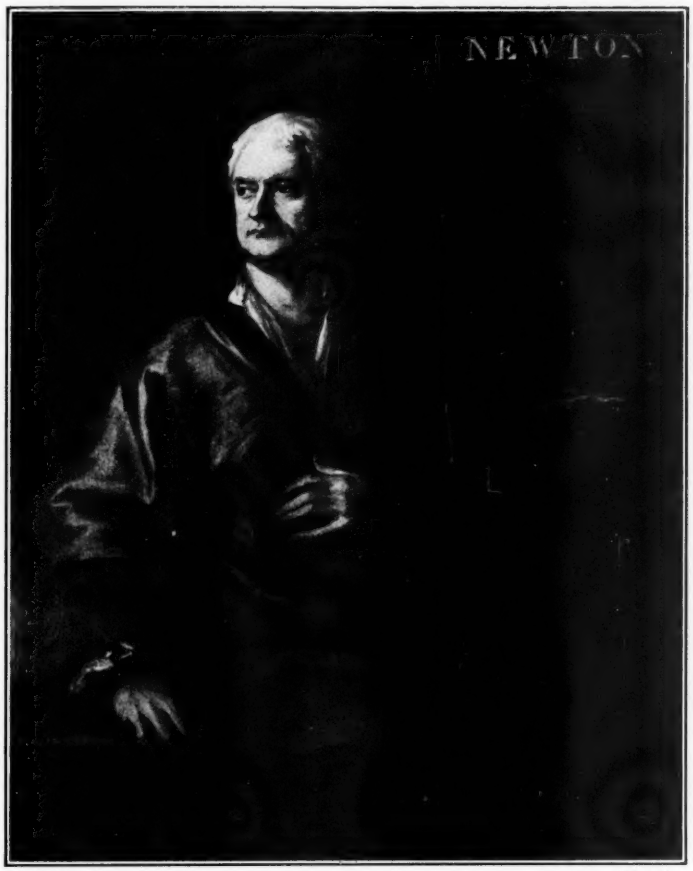
Whence came his genius? We cannot say. His father's family had been settled in this neighbourhood for about a century. His mother was the sister of a local vicar, on whose advice he was finally sent to Trinity College, Cambridge. There seems to have been no trace of mathematical ability in any of his forbears. But, so far as environment went, he was a son of the parsonage. His father died before his birth. His mother's second husband was a clergyman. The boy was thus brought up under clerical influences. Such influences must have done much to give him the puritan austerity, the combined piety, frugality, and generosity, and the interest in theology which he retained throughout his long life. It is a mistake to believe, in consequence of a well-known gibe of Voltaire, that Newton only turned to theology when his powers failed because of the strain of the *anni mirabiles*, the marvellous years, in which the "Principia" was produced. His theological interests, like his piety, persisted throughout his life. We may regret that he did not confine his activity to a domain where his powers were supreme. We must admit that his theological writings were of little permanent value. Yet they show extraordinary width of reading and no little perspicuity. The publication of Newton's tract on the textual criticism of two New Testament passages was deplored by an English Bishop so late as the middle of the nineteenth century. But Newton was correct in his judgment: and the draft revision of the Prayer Book now before the public omits the famous verse of the three heavenly witnesses (1 John v. 7) to which he took exception.

It is well known that Newton refused to take Holy Orders, and that he was only enabled to remain as a fellow of his College while holding the Lucasian professorship at Cambridge by virtue of a special patent from the Crown. There seems to be no reason to doubt that he was urged to take orders in later life, and that, had he done so, he might have received substantial ecclesiastical preferment. To reward a man of science by making him a bishop or a dean seems incongruous to us to-day. But it must be remembered that the connexion of the Church with education and learning was far closer then than now: and the mastership of the Mint, the reward which Newton actually received, was not especially suited to his gifts. Newton's probity and industry while he had care of the coinage have earned the praise of historians and economists: but a lesser man might have done equally valuable work.

Why did Newton, with his high character, sincere piety, and interest in theology, steadily resist ordination? What were his religious views? The question has at times been somewhat acrimoniously debated. But, as a fuller knowledge was gained of the papers preserved in manuscript after his death, it finally became clear that he could not bring himself to accept the Athanasian doctrine of the Trinity. He was an ethical theist with a profound veneration for Christ: but he was not prepared to allow that the Nicene formula adequately expressed the inter-relation of the divine and the human in the person of Christ. As to the miracles of the New Testament, and in particular the virgin birth, he had no doubts. In that, as in his sympathy with Arianism, he was a child of his age. Of that age also were his views of the so-called Mosaic cosmogony of Genesis. In an interesting letter written in 1680, when his intellectual powers were at their zenith, he expressed the opinion that Moses "described realities in a language artificially adapted to the sense of the vulgar." Leibniz at the same time showed a far more modern understanding of the geological process.

When we pass to Newton's metaphysics, and in particular to his views as to space and time, we reach ideas which the theory of relativity has shown to be of transcendent importance. Whence did Newton get the conceptions of absolute space and absolute time which underlie the laws of motion which he formulated? I think it is not to be doubted that they came from the group of divines honourably known as the Cambridge Platonists. The importance of these men in the development of English religious thought has become increasingly clear in our own day, and more especially to English Modernists whose endeavour to re-shape Christian theology merits the close attention which it receives. More and Cudworth were both older contemporaries of Newton: each was living when the "Principia" was written; and More's spiritualised space corresponded closely to Newton's famous scholium. God, as Newton put it, "is not eternity and infinity but eternal and infinite; He is not duration and space, but He continues and is present. He continues for ever and is present

everywhere; and by continuing to be always and everywhere, He constitutes duration and space." In this passage Newton breaks up the space-time continuum: duration and space were separated. But the metaphysic becomes strengthened rather than weakened if, modifying Newton, we say that "by continuing to be always and everywhere God constitutes space-time." Space-time so conceived becomes the primal stuff of the physical universe through which and in which God's purpose of creation is achieved. God thus constitutes the nexus out of which man has emerged.




*Palmer Clarke* [*Cambridge.*

FIG. 2.—Portrait by Thornhill, 1710, in the drawing room of Trinity Lodge, Cambridge.

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It is my purpose to-day to set before you Newton's religious sympathies and thoughts, so I make no attempt to describe the great system of dynamical astronomy of which his laws of motion are the foundation. As it sprang from his brain it was marvellously complete and well-nigh perfect in expression. To make it he invented a new calculus which in his hands was so flexible that, to disarm criticism, he gave it geometrical form. A new era in mathematics began with his discoveries. Instead of slow and painful progress, "line upon line, here a little and there a little," Newton jumped forward; and, as the centuries pass, men will continue to marvel at his splendid genius. In our day a great thinker has made an immortal name for himself. But it is a mistake to think, as some imagine, that Einstein has overthrown Newtonian dynamics; he has rather brought gravitation itself within a modification of his predecessor's scheme. Newton's fame is secure. But he, were he alive, would be the first to praise the achievements of those who carry on his torch of knowledge. Did he not, with genuine humility, liken himself to a boy playing on the sea-shore, now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before him?

The mathematician, like the poet, is born, not made. The science of mathematics is for the few, endowed with special ability which the chances of life permit to be trained and developed. But the conclusions of the mathematician have an interest for multitudes who cannot understand his technique. Newton, in finally establishing the truth of Copernican astronomy, made humanity enter upon speculative inquiries of the highest importance.

What is man's place in the universe? How is he related to the purpose immanent in the whole? Newton gave little indication that he realised how extensively his work bore upon such inquiries, though there is one interesting passage in his

writings where he asks: "If all places to which we have access are filled with living creatures why should all these immense spaces of the heavens above the clouds be incapable of inhabitants?" Reticent though such a sentence is, it was Newton who finally forced the educated world of men to recognise the meanness of man's domicile. The earth had been the centre of the universe: Newton revealed it as a humble satellite of the sun. It needed but recognition of the fact that our sun is but one of millions of stars and man's outlook was transformed. Are there no other stars in the galactic universe which have satellites? We cannot believe it. Are those satellites devoid of life? It is incredible. Has life always taken the same evolutionary course as that which has led to the creation of man upon this earth? Has such evolution never progressed beyond the stage at which man now finds himself? Each question we are practically forced to answer in the negative; and, when we give such answers, what is man in the great scheme of things? Though Newton has been dead two centuries, it is only in our own day that his countrymen, in the mass, are asking the questions to which his discoveries inevitably led.

On the fine statue by Roubiliac which takes the place of honour in the ante-Chapel of Trinity, there is inscribed the line

Qui genus humanum ingenio superavit,

"who in genius surpassed the whole world." It is the tribute of Lucretius to Epicurus, and a greater than Epicurus merits the praise. Wordsworth saw that statue, an inspiration to many an undergraduate before and since his day; and wrote the lines:

The marble index of a mind for ever
Voyaging through strange seas of Thought, alone.

But through stranger seas than Newton ventured has he set men voyaging alone. What is our destiny, as individuals and as a race? Made of dust and water, specks on a small globe, apparently of no special importance in the vast range of creation, we live for but a tick of the astronomer's clock. Light reaches us to-day which began its journey before humanity had appeared upon this earth. Are we, then, of any importance in God's sight? Have we, as individuals or as a race, any permanence? Are we, after all, such stuff as dreams are made of? I give the old answer: "the things which are seen are temporal, the things which are not seen are eternal." The body returns to dust and water; but the mind, which ranges through space and time with a freedom that partakes of the divine, shall not perish. The spirit of a man who strives for truth and seeks goodness belongs to the realm of the eternal.

Here it is fashioned by labour, by self-discipline, by reverence and love. Elsewhere in its perfection it shall have a richer existence. Does it matter that we are little more than point-instants in space-time? I think not; for eternal values are not measured by rods and clocks, nor are they to be found in the blind forces of Nature. "Not by might nor by power but by my spirit, saith the Lord;" and Newton with his simple piety would have accepted the Hebrew prophet's words. As the spirit of man is transformed by the spirit of the Lord, as righteousness and truth fit him to serve God and think His thoughts, he becomes a Son of God and an inheritor of the Kingdom of Heaven.

## Newton's Place in Science.

By F. S. Marvin.

TWO men in science have attained a world-wide position and stamped their names on periods marked by the triumph of their theories: and only two. Both were Englishmen, and Newton's position is even clearer and less disputed than Darwin's. Descartes left behind him a Cartesian school and a Cartesian philosophy, but he is not remembered as one of the most eminent men of science. Archimedes and Galileo, the two names one might put beside Newton's as men of science, did not produce a striking, conclusive, and all-embracing theory, such as that which made Newton famous and founded a 'Newtonian' synthesis to last without question or modification for two hundred years.

Though Newton's position is thus unique, it must be studied in connexion with that of others and in its right place in the history of thought. For science being the central thread in the history of mankind, great men of science can less than any other type be isolated and treated as individual phenomena. A Michelangelo, a Shakespeare or a Beethoven, even an Alexander or a Napoleon, may be more legitimately thus isolated and wondered at, but never a man of science; and Newton, like the rest, brought to fruition the labours of others and was himself surrounded by many men close in pursuit of the same truth, dimly perceived, but by him to be clearly articulated and established. This was his special quality as a thinker and a man of science. He was a mathematician, primarily a

geometrical mathematician, and he applied a mathematical mind of the most powerful order to the elucidation of problems which were being approached at that time on lines of somewhat vague and quasi-philosophical speculation.

Newton's relation to Descartes is typical. He studied him eagerly as a young man, and was more influenced by him and by Wallis's "Arithmetica Infinitorum" than by any other writers. But his reaction was always critical. Descartes, besides his "Géometrie" published in 1637, elaborated, in his "Principia" of 1644, a physical theory of the universe which stimulated strongly the growing tendency of thought to explain all celestial phenomena by the same mechanical laws which, thanks to Galileo's experiments, had been shown to hold good for phenomena on earth. Descartes therefore must rank as a co-founder, but his work was vitiated by 'metaphysical' reasoning—that is, the assumption that certain results must, by the 'nature of things,' be what investigation had never proved them to be, or had actually proved them not to be. Such were Descartes's assumptions of a number of vortices in the centres of which the sun and planets were swept round, the secondary whirlpools in which the planets moved causing variations of density in the major whirlpool of the sun, and hence deflecting them from their natural circles into ellipses. Newton confronted these imaginary conclusions with the facts observed and the laws deduced from them by Kepler and Galileo, and showed their inconsistency. This appears in the second book of Newton's "Principia," published in 1687 but written long before. He was of the lineage of Archimedes and Hipparchus, and not of Aristotle, and his coincidence with Galileo's work on bodies falling to the earth is the true link in the formation of the first and most powerful of all physical syntheses.

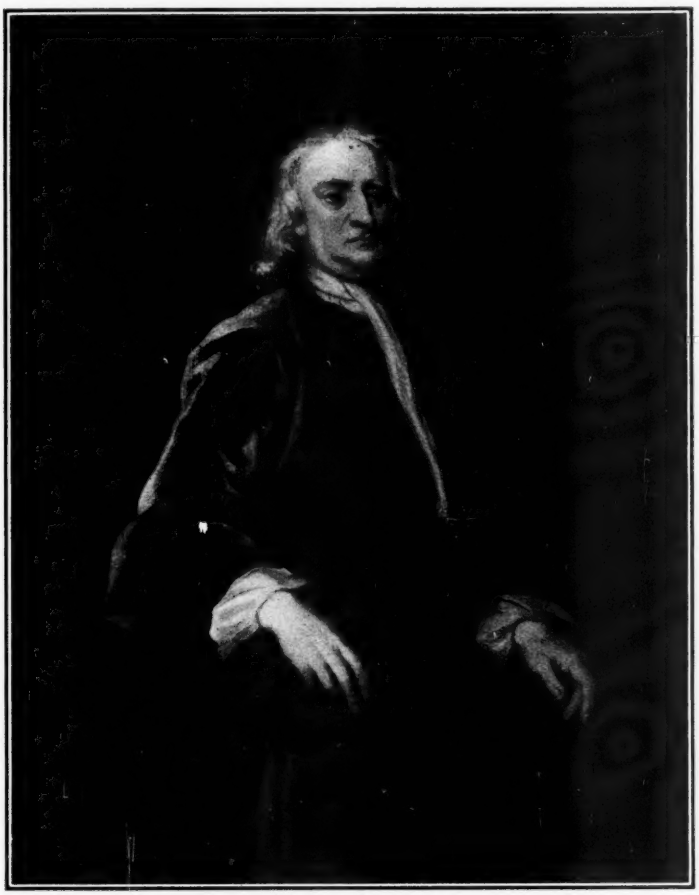

FIG. 3.—Portrait by Vanderbank, 1726, in the meeting-room of the Royal Society, over the president's chair, and given by Mr. C. B. Vignoles in 1841 to the Society.
Reproduced by permission of the Society.

Kepler had established his three laws of planetary motion by 1619, but he had been unable to connect them with the law of falling bodies on the earth. His notion of gravity was the quite vague one of the mutual tendency of similar bodies to unite, and in the introduction to his work on Mars he actually committed himself to the mistaken idea that "if the earth and moon were not kept at their respective distances they would fall one on the other, and, supposing them of the same density, the moon would pass through $\frac{53}{54}$ of the distance, the earth through the remaining part."

Then came Galileo's work at Pisa and Padua, of which the full account was published in the "Dialogues on the Two Sciences of Mechanics and Motion" in 1638, though the capital conclusions had been reached some years before. It was in

bringing together and proving the identity of Kepler's and Galileo's laws that Newton's genius shone forth. To guess that there was a connexion was easy and almost inevitable for any one who brought the two sets of facts together; to show it accurately and irrefutably was Newton's greatest work. It happened sometime between 1665 and 1679, but the solution was not passed on to others until the autumn of 1684. In the August of that year Halley, who, with several others, had been puzzling at the problem, went to Cambridge to consult Newton about it. Hooke, Huyghens, and Wren, as well as himself, were, he said, all working on the theory that the force of the attraction of the sun or earth on an external particle varied inversely as the square of the distance, but they could not from that law deduce the orbits of the planets. Could Newton find out what the orbit of a planet would be, if the attraction were as the inverse square of the distance? Newton replied that it was an ellipse and that he had worked it out in 1679. He promised to recover it from his notes or his memory and sent it in to Halley in November. On receiving it Halley went again to Cambridge and at this second visit set up those relations between himself and Newton which enabled him to extract the "Principia" from its reluctant author. Halley's was the persuasion and Halley's the financial guarantee. Under this stimulus the first book of the "Principia" was finished by the summer of 1685 and presented to the Royal Society on April 28, 1686. The second book was also completed in the summer of 1686 and deals with hydrostatics and hydrodynamics—motion in a resisting medium—and gives the criticism of the Cartesian theory of vortices to which allusion was made above.

It is interesting to note the similarities in the genesis of Newton's other most famous achievement—the infinitesimal calculus—which he knew as the 'method of fluxions.' Here again many minds were at work, and they were working on an ancient problem to which the Greeks had made a geometrical approach and Descartes had brought much nearer by his approximation of algebra and geometry. Of Newton's contemporaries, several, including especially Fermat, Wallis, and Barrow, had foreshadowed a calculus, by which infinitesimal quantities might be summed and infinitesimal differences in varying quantities measured. Newton's greatness was shown by welding together from the various indications he received from others a sound and practical method which he used for his own purposes so early as 1666, but did not publish until 1693. In its reception he had not the same good fortune as with the law of gravitation, for there was no Halley to steer his ship into port. On the contrary, thanks to his long delay in publication and to the fact that some of his unprinted papers came into the hands of Leibniz in 1676, there was always a doubt as to the priority and independence of Leibniz' method, which was actually printed in 1684. No one need reopen the question now, though it was disputed long and bitterly in the last years of Newton's life. Of Newton's own originality—in the only sense in which a man can in such a matter be original—there is no question; he invented for himself a 'method of fluxions' with his own notation, and it is also unquestioned that Leibniz' method of notation was the more convenient and gained general acceptance. In fact, England, remaining more Newtonian than Newton, was left in the rear-guard of mathematics for more than a hundred years. We may well believe, however, that two great men came by rather different paths to similar, but not identical, conclusions, on a practical question, small in form but big with consequences, on which the ablest mathematical brains of the seventeenth century had been all converging.

It is not within the scope of this article to deal with the modifications in the Newtonian system as a whole which speculations in our own days have introduced; it must suffice to point out the magnitude of the results Newton achieved, and one or two general qualities in his way of thinking, partly characteristic of his time, partly peculiar to himself. These have much to do, first with the enormous success and dominance of the system for more than two centuries, and then with the reaction.

The seventeenth century was the flowering age of mathematics, as the eighteenth was of chemistry and the nineteenth of biology, and the last four decades of the seventeenth saw more forward steps taken than any other period in history. The fact that these developments of mathematics were in the direction of physics, and had so large an effect in the world of practice, was due to Newton's mechanical bent. He had been as a boy fond of making mechanical models, and it was not until later that he took up mathematics theoretically. Euclid was an interesting novelty to him when he went to Cambridge at the age of eighteen, and by that time he could see the truth of the propositions intuitively and went on to Descartes for further light. The great work of those closing decades of the century was predominantly that of Newton, and it included, beside the two fundamental discoveries which we

have mentioned, improvements of all kinds in algebra and analytical geometry, and the satisfactory formulation of dynamics, statics, and hydrodynamics with the modern apparatus of calculation. His extreme aptitude for applying a mathematical, geometrical mind to external Nature led to his theory of the propagation of waves and his researches in optics.

This was the leading characteristic of Newton's mind and these its chief fruits. But he was attached on another side to the mystical philosophy of his time, and this has recently been studied in a curious work which shows the possible connexions of a theory comprising pure laws with the wildest theological and metaphysical speculations.[1] Newton himself was scrupulously scientific in his deductions: "We must always," he said, "find the laws first, before we speak about their causes." But he was contemporary with the school of Cambridge Platonists and mystics — More, also of Grantham, and Clarke and Cudworth. To them the unknown side of gravity and the concepts of 'ethereal spirits' or 'effluvia' provided a field for the play of unbased and unbridled fancy. In this Newton also often plunged, and it is the strongest testimony to the solidarity of his mind that he never allowed such speculations to deflect in the smallest degree the correctness of his reasoning. Being dissatisfied with the attraction of bodies at a distance, as the final explanation of gravity, he assumed in his own mind the existence of a 'spiritus' or active principle which "penetrates to the very centres of the sun and planets without suffering the least diminution of its forces. . . . For, seeing that the variety of motion in the world is always decreasing, there is a necessity of conserving and recruiting it by active Principle, such as are the cause of gravity. This Principle would be God, and Space is the medium through which God can come in contact with finite objects and creatures —his boundless and uniform Sensorium." Thus the mystery of action at a distance becomes in the light of a mystic philosophy the meeting point of material bodies and spiritual forces.

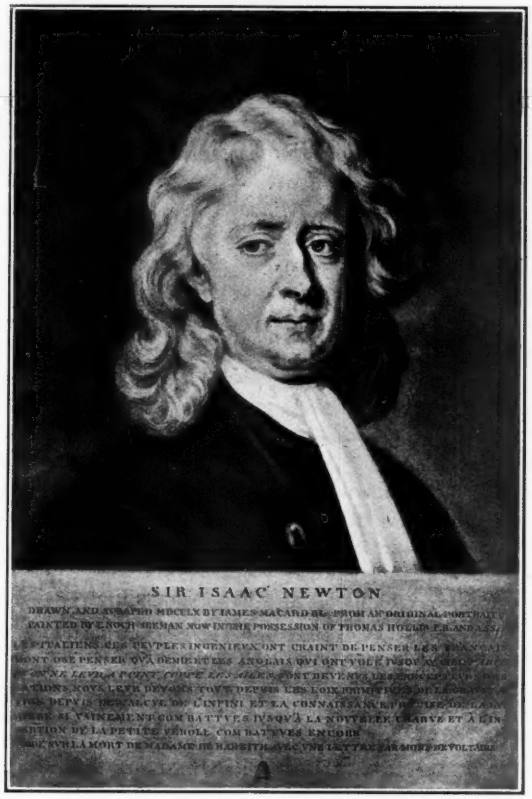


FIG. 4.—Portrait by Enoch Seeman in the dining-room of Trinity Lodge; engraved by MacArdel, 1760.
Reproduced from an engraving in the possession of the Royal Institution.

It is well that these thoughts should be revived and borne in mind, not in any sense as a derogation from the strictly scientific nature of Newton's work, but as pointing three important morals in the general history of thought, as well as the position of Newton in modern science. First, that every thinker is the child of his times and cannot be dissociated from them or understood without them. Newton's philosophy, including therein its science, fits in perfectly with the theology of "Paradise Lost," which was published in 1667, the year after Newton's own *annus mirabilis* of gravitation and calculus. Secondly, that it is quite possible to combine in the same brain the most rigidly positive doctrines about certain aspects of phenomena with highly speculative and undefinable theories about others. Newton is the noblest example of the mind which can shut the door of its laboratory, and leave instruments and notes of research intact, while it proceeds in its oratory to speculate about the unknown and write numerous works on divinity and prophecy. Thirdly, that this absolute and undescribable sphere of Newton's thought, while it does not affect the validity of his conclusions so far as they go, made it certain that revision would come in the fullness of time. That revision began

[1] A. J. Snow, "Matter and Gravity in Newton's Physical Philosophy" (Oxford, 1926).

soon after the bi-centenary, not of his death, but of the completion of his capital works. It has not impaired their greatness, but, completing and putting them in their true setting, it has enhanced their glory and shown their author not as an isolated and immutable genius but as a mighty branch of an ever-growing tree.

One strange paradox and error has come with the whirl of time. The name of the most deeply religious and theological of all great men of science is now often held to describe a purely mechanical, materialistic, and unspiritual hypothesis of the universe. By eliminating the unknown, and attaching the hypothetical forces to matter itself, the system could be turned in this direction. But it is an undeserved and unexpected fate for the author of "The History of the Creation."

## Isaac Newton.[1]

By J. H. Jeans, Sec. R.S.

WE are met to do homage to the memory of one who was certainly the greatest man of science, and perhaps the greatest intellect, that the human race has seen. We can claim this position for him without arrogance; it is not only accorded him by his own countrymen and co-workers in science, but also by those most capable of forming an unbiassed judgment. Laplace considered that the "Principia" had assured to it for all time "a pre-eminence above all other productions of the human intellect." Lagrange used similar terms, describing it as "the greatest production of the human mind," and confessing himself dazed at such an illustration of what man's intellect could achieve. He added that Newton was not only the greatest genius that had ever existed, but also the most fortunate, for as there is but one universe it can but happen to one man in the world's history to be the interpreter of its laws. Voltaire wrote of him that "if all the geniuses of the universe were assembled, he should lead the band."

As British men of science we may rejoice that this greatest of all men of science was British. We may feel the satisfaction of the French soldier when he thinks of Napoleon, or of the Italian sculptor when he thinks of Michelangelo, the satisfaction of knowing that the greatest man in our own calling was our own countryman. But we may experience also a wider satisfaction. In the words of his monument in Westminster Abbey, "Sibi gratulentur mortales tale tantumque extitisse humani generis decus," we may rejoice that there has appeared so great a glory to the human race.

This afternoon we are to make a pilgrimage to the house which is doubly distinguished as being the birthplace both of the man and of his greatest achievements. This morning we are met in the school in which he acquired the rudiments of knowledge to contemplate the vast extensions that knowledge received at his hands. But before turning to his life's work, let us glance for a moment at his life.

We see him first as a child of delicate health, having indeed been so feeble and undersized at birth that it was scarcely hoped that he could live. At the age of eleven he is sent to school here. He is, on his own admission, far from industrious, and does not in any way distinguish himself at first. But one day, being maltreated by a senior boy, he decides to fight, and wins. This awakens a spirit of emulation; henceforth he applies himself to his studies with a new zest, speedily outstripping his enemy, and in time reaching the top of the school.

We may speculate as to what would have been the effect on the course of his life, and on the history of science, had it not happened that a big boy lost his temper nearly three centuries ago and kicked a small boy, perhaps in this very room. For myself I imagine it would have all been much the same. Whatever he may have done, or not done, in lesson time at school, he spent his leisure time in scientific and mechanical occupations, in making model windmills and water-clocks, or in estimating the force of the wind by the ingenious method of measuring how much farther he could jump with it than against it; he found less pleasure in flying his kite than in experimenting as to the best length of tail and the best point at which to attach the string. When he was taken from school at the age of fourteen to help on the family farm at Woolsthorpe, his mind refused to be tied down to the dull routine of crops and herds, and turned, as though to its natural element, to problems of mathematics and mechanics. When he was driven from Cambridge to Woolsthorpe by the plague, he sat in the garden and "fell into a speculation on the power of gravity"; he "began to think of gravity extending to the orb of the moon," with results which are familiar to all the world.

[1] Opening address delivered on March 19, in the Old School, King's School, Grantham, at the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

The most outstanding feature of his personality was, I suppose, the intense sincerity of his search for truth. Like all single-minded investigators of Nature, he was continually overwhelmed at the thought of the comparative smallness of his achievement. In the light of all that is known as to the transparent honesty and unaffected simplicity of his character, his oft-quoted comparison of himself to "a boy playing on the seashore, whilst the great ocean of truth lay all undiscovered before me," must be regarded as a perfectly sincere statement of how his great triumphs appeared to him. This modest simplicity, combining with an over-sensitive and profoundly moral nature, produced two outstanding results in his scientific life. In the first place, he suffered fools far too gladly, and spent more than enough time and energy explaining his scientific position to critics who had neither the knowledge nor the capacity to understand. In the second place, he was over-sensitive to criticism, and perhaps over-ready to discover insincerity, unfairness, or intellectual dishonesty in his opponents. He had such an abhorrence of these qualities that the bare suspicion of them infuriated him, so that at times he showed neither chivalry nor common courtesy in controversy.

There has been much discussion as to what must be deemed to be the outstanding qualities of Newton's intellect and genius. First and foremost must of course be placed his stupendous mental grasp and breadth, his capacity for seeing all aspects of a question and so making unerringly the right choice between alternatives which would have seemed equally probable to a less powerful mind. With his accustomed modesty, which seems to have amounted almost to ignorance that his talents were in any way exceptional, he declared that "if he had done the world any service, it was due to nothing but industry and patient thought." So it may have seemed to him, but obviously his genius must have been something more than an infinite capacity for taking pains; this could neither explain the gigantic amount of his scientific output nor the terrific speed with which his results appeared. We must surely add, in extreme measure, the much rarer capacity for not taking pains, or wasting time, over lines of thought that lead nowhere, the capacity of choosing the right course by a clear vision emanating from sound knowledge. In this quality, which is often described by the superficial terms of 'sagacity' or 'ingenuity,' he was supreme: this is the aspect of him which is commemorated on his statue in Trinity Chapel: "Newton qui genus humanum ingenio superavit."

Apart from this, his amazing success must, I think, be attributed largely to the 'all-roundness' of his mental equipment. This exhibited no *lacunae*, his mind working with equal ease and certainty over the whole range from mathematical abstractions to mechanical details. Biot puts it only slightly differently when he says, "Comme géomètre et comme expérimentateur Newton est sans égal; par la réunion de ces deux genres de génies à leur plus haut dégré, il est sans exemple."

There are parts of his works which one cannot read, even now, without being struck, often as by a blinding flash, with the intense modernity of his thought. The artist attributes modern feeling to, say, Bach or Michelangelo, because their complete mastery of their arts led them to perceive, at least in outline, almost everything which these arts were capable of expressing; in a sense music and sculpture do not so much advance beyond Bach and Michelangelo as oscillate round them. With obvious limitations the same is true of Newton. So clearly and so completely had his mind grasped the essential outlines of the physical universe that, time after time, we find his unerring thought travelling much the same path and encountering much the same difficulties as the scientific investigator of to-day.

A striking example appears in the first few pages of the "Principia," where Newton discusses the difficulty of distinguishing between relative and absolute motion, conjecturing that possibly "in the remote regions of the fixed stars, or perhaps beyond them, there may be some body absolutely at rest," but that "absolute rest cannot be determined from the positions of bodies in our regions." Since Newton wrote this the fixed frame of reference of the luminiferous ether has come and gone, and what might have been considered antiquated thought a few years ago could well be transposed bodily into a treatise on Relativity to-day. Similarly, Newton's theory of 'fits of easy reflexion and transmission,' which seemed foolishness until recently, is now seen to be closely allied in principle to Einstein's 'probability-coefficients' for the interchange of energy between atoms and radiation, while his support of a corpuscular theory of light has acquired a new significance from the modern theory of light quanta. As a final example we find him writing to Bentley (Dec. 10, 1692): "It seems to me that, if the matter of our sun and planets and all the matter of the universe were evenly scattered . . . throughout infinite space,

it would never convene into one mass; but some of it would convene into one mass and some into another, so as to make an infinite number of great masses, scattered great distances from one to another throughout all that infinite space. And thus might the sun and fixed stars be formed, supposing the matter were of a lucid nature." I doubt if modern cosmogony can give a much better account of the genesis of the stars.

So, although two centuries have elapsed since Newton died, we may expect to hear this morning not only of work accomplished more than two centuries ago, but also of scientific methods and lines of thought that seem entirely alive to us to-day.

## Newton's Work in Astronomy.[1]

By Sir Frank Dyson, K.B.E., F.R.S.

WRITING in the year 1738, Voltaire narrates that Madame Conduitt, Newton's niece, had related to him that in the year 1666, Newton, seeing an apple fall in the garden of Woolsthorpe, meditated profoundly on the cause which draws all bodies in a line which, if produced, would pass through the centre of the earth. Voltaire then tries to reconstruct the line of thought which led Newton to the law of universal gravitation. Would it act if the apple were a mile high or ten miles high? If so, might it not act so far away as the moon, and this power, whatever it is, be the same as that which keeps the moon in its orbit round the earth, the planets moving round the sun, and its satellites round Jupiter?

Vague ideas were current as to the forces which kept the planets in their orbits. Descartes put forward a theory of vortices in a fluid medium diffused through space. This gave a possible sort of mechanism suggested by water or air whirling on the earth, but the details could not be fitted to agree with the known facts of astronomy. Kepler had suggested an attraction between the planets varying as the inverse square of the distance and had afterwards substituted the inverse distance. Others before Newton may have suggested the inverse square, but were unable to carry the idea further. Newton was able to bring the hypothesis to the stage at which it could be tested by its agreement with phenomena. Assuming that the force by which the earth attracted outside bodies varied inversely as the square of the distance and at the earth's surface caused a body to fall 16 feet in one second, he calculated that, as the moon is sixty times as distant from the centre of the earth, the attractive force of the earth at that distance would pull the moon inwards towards the earth 16 feet in one minute. He then calculated the actual force necessary to keep the moon moving round the earth in 27 days 7 hours at a distance of sixty times the radius of the earth. It is said, but without much evidence, that Newton used the erroneous value of 60 miles for 1° of latitude of the earth's surface employed by mariners to calculate the earth's radius, and was unacquainted with the more accurate measures of Norwood in 1636, which made 1° of latitude 70 miles. He thus obtained a value one-seventh part too small, and in view of this discrepancy for a time turned his thoughts to other things. He may have returned to the subject a few years later when he became acquainted with the results of Picart's triangulation from Paris to Amiens made in 1669–1670.

Though we do not know the precise date when Newton had satisfied himself of the truth of the law of gravitation, in 1684 it was complete, and he could not only demonstrate to Halley that under a law of the inverse square planets would describe ellipses round the sun, but could also promise to send to the Royal Society the mathematical principles of natural philosophy in which the doctrine of universal gravitation is set forth in order and its manifold consequences deduced.

In Book III. of the "Principia" Newton begins with the astronomical evidence. The four satellites of Jupiter describe equal areas about Jupiter in equal times; and the squares of their periodic times are proportional to the cubes of their distances from Jupiter's centre. The same is true of the satellites of Saturn. The same holds for the motion of the planets round the sun. The moon, too, if the small disturbances caused by the sun are neglected, describes equal areas in equal times about the earth's centre. It follows from the mathematical demonstration in Book I. that the forces which keep Jupiter's satellites in their orbit tend to Jupiter's centre and are inversely as the square of the distances of the satellites from that centre. The same applies to Saturn and its satellites. The movements of the planets round the sun are known to obey these laws of Kepler's with very great accuracy. Consequently this law of the inverse square holds with equal accuracy. The moon, however, does not move accurately in an ellipse,

[1] Address delivered on Mar. 19 at Grantham in connexion with the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

as its apse shifts 3° in each revolution, but this is to be explained by the disturbing action of the sun; but making allowance for this, the force attracting the moon to the earth varies according to the same law of the inverse square of the distance. With the accurate distance of the moon from the earth's centre, the distance it is drawn towards the earth in one minute is calculated, and thus the amount of the earth's attraction at the moon's distance is found. This will be increased $60^2$ times at the earth's surface. It is then found to agree exactly with the force which causes bodies to fall at the earth's surface. Thus the force which retains the moon in its orbit is at the earth's surface identical with the well-known property of weight or gravity.

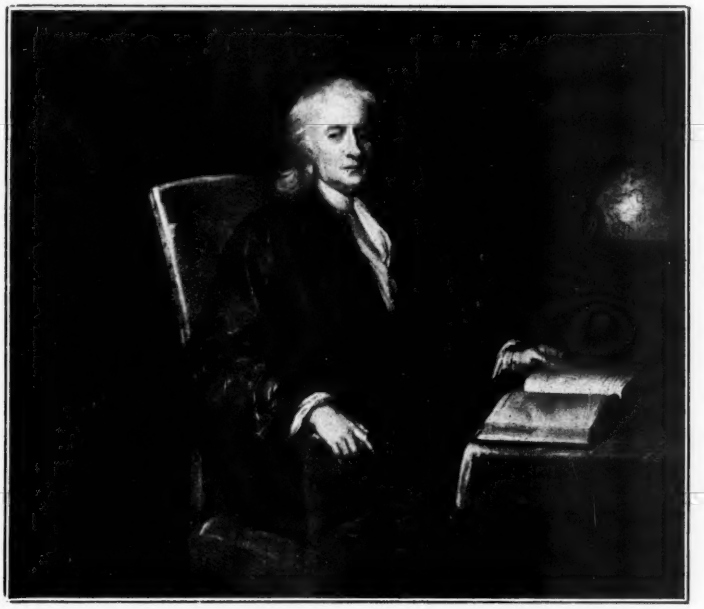

Fig. 5.—Portrait by John Vanderbank in the National Portrait Gallery, London.

It is, then, gravity which keeps the planets and satellites in their orbits, and all gravitate mutually towards one another, with forces which vary inversely as the square of the distance between their centres. Newton now investigates whether he can detect any differences in the gravity of different substances. He provided two wooden boxes round and equal; filled one with wood, and suspended an equal weight of gold in the centre of oscillation of the other. The boxes, hanging by equal threads of 11 feet, made a couple of pendulums perfectly equal in weight and figure and equally receiving the resistance of the air. In this way he found that the force of gravity is within 1/1000th part the same for gold, silver, lead, glass, sand, common salt, wood, water, and wheat. That is, if the resistance of the air were absent, all those bodies would fall at the same rate.

Newton shows point by point that gravity is a universal property of all bodies, dependent solely on the amount of matter they contain. This must hold for the planets, and every part of each planet must gravitate to every part of any other planet, and finally every particle of matter attracts every other particle with a force proportional to the product of their masses and inversely proportional to the square of the distance between them. He then shows that two uniform spheres will attract one another as if their masses were concentrated at their centres. This is perhaps the most splendid piece of geometry ever achieved. It appears to have cost Newton a great deal of thought. He says:

"After I had found that the force of Gravity towards a whole planet did arise from and was compounded of the forces of gravity towards all its parts, and towards every one part was in the reciprocal proportion of the squares of the distances from the part, I was yet in doubt whether that reciprocal duplicate proportion did accurately hold or but nearly so, in the total force compounded of so many partial ones; for it might be that the proportion which accurately enough took place in greater distances should be wide of the truth near the surface of the planet, when the distances of the particles are unequal and their situation dissimilar. But by the help of prop. 75 and 76, Book I., and their corollaries, I was at last satisfied of the truth of the proposition as it now lies before us."

The results which follow from the law of gravitation set down in its generality are almost magical. The laws of the movements of the planets round the sun and satellites round the planets follow immediately. Comparison of the dimensions and rate of description of the orbits give the comparative masses of the sun, earth, Jupiter, and Saturn. The sun is 1060 times as massive as Jupiter and 270,000 times (with the parallax Newton used) as

massive as the earth. Thus the earth is found to have a mean density of four times that of the sun, and Newton hazards the guess that the earth's density is between five and six times that of water. The effect of the mutual action of the planets on one another is indicated, and the disturbing action of the sun on the moon is shown to give rise to the revolution of the apse and node, the variation and other inequalities of the moon's orbit. The inequalities known to astronomers were explained and new ones predicted. This complicated problem was carried so far as possible by geometrical methods. Newton said he should give up thinking about the moon, for it made his head ache.

The figure of the earth is next shown to be a spheroid. Owing to rotation the equatorial diameters of planets must be greater than their polar diameters. Observation showed that this was true in the case of Jupiter, which revolves very rapidly in 9 hours 50 minutes. By considering the equilibrium of water in two channels from the pole and a point on the equator to the centre of the earth, Newton showed that the equatorial diameter of the earth would be 1/230th part greater than the polar diameter. Assuming a spheroidal form for the earth, he calculated the length of a degree in different latitudes, and the length of a pendulum beating seconds. He compared these with observations made in various places, such as Paris, Cayenne, St. Helena. This investigation served as a great stimulus and provided a theoretical basis for the more accurate determination of the size and figure of the earth.

The general explanation of the tides was a further deduction from the law of gravitation. Newton showed that the attraction of the moon gave rise to forces tending to heap up the water at a point of the ocean directly under the moon and also at the antipodal point; similarly the sun's attraction tended to cause high water directly under the sun and at the antipodal point. He showed how the spring and neap tides would occur, how the position of the sun or moon, north or south of the equator, would affect the tides and how they would vary in different latitudes. By comparing the heights of the spring and neap tides he concluded that the tide-raising force of the moon was 4·48 times that of the sun. Knowing the mass of the sun he finds it should raise a tide of 1 ft. 11 in. in the open sea, and infers for the moon one of 8 ft. 8 in. Newton gave only an elementary theory of the tides, indicating the causes which produced their main features, and left to his successors the difficult problem of a dynamical theory.

Just as the moon produces tides on the earth, so the earth would produce tides on the moon. As the period of revolution of the moon is the same as its period of rotation on its axis, the moon always presents the same face to us. If the moon were covered with water, the water would be heaped up at the part facing the earth and the part diametrically opposite. So Newton infers that the moon has a spheroidal figure, the longer axis pointing to the earth, and being 186 feet in excess of the equatorial diameter.

The precession of the equinoxes is explained as due to the attraction of the sun and moon on the spheroidal part of the earth. Newton had shown how the attraction of the sun on the moon produced a movement of the nodes of the moon's orbit. In other words, a line drawn perpendicular to the plane in which the moon is moving will trace out a cone in 18½ years. He deduced what would be the movement of the nodes of the orbit of a little moon near the earth's surface, revolving in a sidereal day. The same thing would happen, he says, to a ring of moons, whether they did not eventually touch each other or whether they were molten and formed a continuous ring. Next let the ring of moons be attached to the sphere and communicate its motion. Thus the axis of a mass composed of the ring and sphere together will have movement due to the sun of 9″ annually, and the effect of the moon is in the proportion of the tide-raising force of the two bodies. Airy remarks: "If at this time we might presume to select the part of the 'Principia' which probably astonished and delighted and satisfied its readers more than any other, we should fix, without hesitation, on the explanation of the precession of the equinoxes."

A good deal of space in Book III. is given to comets. Tycho Brahe had shown that they moved in the celestial spaces and did not belong to our atmosphere. Newton regarded them as bodies like the planets, but moving in very elliptic, nearly parabolic orbits, and showed how from three observations their paths might be determined. The tails arise from the atmosphere of the heads, their great length and splendour being due to the heat which the sun communicates to the comet when near to it. He notes that the perihelion distance of some comets is very small, that of 1680 being only one-sixth of the sun's diameter from the sun's surface. Owing to resistance it is possible they may, in course of time, fall into the sun. He hazards the suggestion that fixed stars may be recruited by the fall of comets into them, and here

may be a possible explanation of the new stars which have appeared from time to time.

There are two other contributions of Newton to astronomy to which I should like to make a brief reference. In the "System of the World," a work which Newton intended at one time to form Book III. of the "Principia," there is a little-known estimate of the distance of the fixed stars. As it was one hundred years before any better estimate was obtained, I venture to direct attention to it.[2] The disc of Saturn, Newton says, is 17″ or 18″ in diameter. It receives 1/2,100,000,000th of the sun's light. If we suppose Saturn to reflect $\frac{1}{4}$ of the light it receives, the whole light reflected from its illuminated hemisphere will be 1/4,200,000,000th of the light emitted from the sun's hemisphere. If the sun were $10{,}000\sqrt{42}$ times more distant than Saturn, it would yet appear as lucid as Saturn now does without its ring. Let us, therefore, suppose that the distance from which the sun would shine as a fixed star to exceed that of Saturn by about 100,000 times. This gives 0″·2 as the annual parallax of a first magnitude star and 0″·002 as its diameter. Newton answers the objection that light may be absorbed in its passage through space, for in that case the remoter stars would scarcely be seen.

[2] Schlesinger, *Publ. Astr. Soc. Pacific*, vol. 14, p. 170.

We may also recall the fact that Newton was the first person who actually constructed a reflecting telescope. He gives an account in the *Phil. Trans. Roy. Soc.* of the chemical experiments he made for the best composition of speculum metal. The use of pads of pitch on which rouge is spread for polishing mirrors and lenses was invented by him. I believe instrument-makers have found no better materials since.

Returning to Book III. of the "Principia," nowhere has so much been comprehended in so few words as in the law of gravitation. It does not explain to us why the moon and planets move as they do, but it does show us how they move and enable us to calculate their movements. Generations of mathematicians and astronomers have employed large parts of their lives in tracing its consequences in most intricate details, comparing the results with observations, and discerning the past and future history of the solar system. Laplace, perhaps the most illustrious of Newton's followers, says "the number and generality of Newton's discoveries relative to the system of the world, the multitude of original and profound views, which have been the germ of the most brilliant theories of the geometers of the last century, will assure to the 'Principia' a pre-eminence above all other productions of the human intellect."

## Newton's Work in Mechanics.[1]

By Prof. Horace Lamb, F.R.S.

IF we wish to realise the extent and importance of Newton's contributions to the science of Dynamics, the most obvious way is to compare the long series of propositions contained in the first two books of the "Principia" with the brief record of what had been accomplished by his predecessors and contemporaries. To both he makes ample acknowledgment. Galileo, freeing himself from scholastic tradition, and appealing to experiment as the ultimate test, had ascertained the laws of falling bodies and, assuming the composition of motions, had deduced the parabolic path of a projectile. Considering, further, motion on a smooth incline, he had proved that the velocity acquired from rest must depend only on the vertical height fallen through, and had supported this theoretical result by an ingenious experiment. It is remarkable that this particular result should have been adopted by Huyghens as the basis of his treatment of the cycloidal pendulum instead of the more natural method of resolution of forces, which we should expect. Huyghens, for some reason, seems not to have been satisfied with Galileo's demonstration, and argued that if the theorem did not hold it would be possible for a particle descending one incline and ascending another to pass from rest to rest at a higher level. We have here, of course, a partial apprehension of the principle of energy. His work on the compound pendulum, the first and for a long time the only example of what we call Rigid Dynamics, was based on a similar argument, the assumption here being that a system of particles cannot of itself move from rest in such a way as to raise its centre of gravity. Huyghens, again, had published (without proof) his theorems on centrifugal force, and simultaneously with Wren and Wallis had investigated the laws of collision of bodies. He has the distinction of being mentioned by Newton more than once, with especial respect.

This brief outline embraces, I think, almost all that is important from the point of view of pure Dynamics. The advance in knowledge within a period of little more than half a century was, it is

[1] Address delivered on Mar. 19 at Grantham in connexion with the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

true, remarkable, and still more the changed spirit of investigation, with the ultimate appeal to experiment rather than to *a priori* reasoning. Yet although the laws of motion laid down by Newton were, as he asserts, accepted, particular problems were treated separately, each in a special way. There was no systematic procedure based on general principles by which any given dynamical problem could be reduced to a question of mathematics. For example, no case of a *variable* force was attempted, and indeed the whole theory of curvilinear motion, if we except the parabola of Galileo and the cycloid of Huyghens, was unexplored. The mathematical methods also were rudimentary. The geometrical reasoning of Huyghens, for example, though most ingenious, is special to his problem, and laborious.

When we turn to the "Principia," we find a long array of consequences deduced from the fundamental laws, and by appropriate methods, especially by the consideration of the effect of force on the curvature of an orbit. If we omit from the first eleven sections of the first book a few propositions of mainly geometric interest, and those which are technically astronomical, we find in substance the whole structure of Particle Dynamics much as it is set forth in modern text-books, except for the mathematical methods employed. If we were to reverse what is supposed to have been Newton's procedure and reconvert his geometry into analysis, the result would be a perfectly adequate treatise. This is, in fact, very much what has happened; successive writers have introduced modifications and developments here and there, but the essentials remain the same.

The leading motive which gives a sort of unity to the first book is the law of gravitation with its simpler consequences. We find accordingly the theory of motion in a conic about a focus, the determination of orbits under given conditions, and even a graphical construction for the time in the orbit. The elliptic problem had been put to Newton by Halley, but no one apparently had even thought of the parabolic and hyperbolic cases, though these were to have an astronomical importance. The first step towards a theory of perturbations is made in the theory of revolving orbits, and the calculation of the motion of the apse when the orbit is nearly circular. Next, abandoning the notion of a fixed centre, Newton solves the problem of two bodies subject to their mutual gravitation, with the correction to Kepler's third law. This chain of deductions is brought to a culmination in his celebrated proposition 66, with its long series of corollaries, where the problem of three bodies is attacked in the special form in which it usually presents itself in astronomy. The notion of 'disturbing force,' so fundamental, for example, in the theory of the tides as well as in gravitational astronomy generally, here appears for the first time.

*W. Lee* [*Grantham.*

FIG. 6.—Manor House, Woolsthorpe, the birthplace of Newton.

Although the main theme, as has been said, is the law of gravitation, Newton's instinct for generalisation and for mathematical elegance leads him to discuss various collateral questions which incidentally come within the scope of his methods. For example, the whole theory of central orbits under various laws of force is discussed and practically exhausted, except for the interesting case of the inverse-cube, which was left to be completed by Cotes. The law which comes next in importance to the inverse square, namely, that of the direct distance, with the theory of harmonic motion, elliptic and rectilinear, is fully treated. A number of propositions are devoted to various cases of constrained motion, and we meet here

with the elegant theorem of the isochronous property of oscillations on an epi- or hypo-cycloid under a central force of this kind. It is, I think, characteristic of Newton, wishing to do things in his own way, that Huyghens' famous result for the ordinary cycloid should be introduced merely as a corollary to this. The first book closes with the theory of attractions of bodies of finite size, including the classical cases of the sphere and spherical shell. Some final propositions on the attractions of stratified media are introduced for their bearing on the corpuscular theory of light.

The second book of the "Principia" deals with a number of questions belonging to the mechanics of fluids, and specially with the theory of resistance. After treating a few standard problems, such as we find in our text-books, on motion under resistances proportional to various functions of the velocity, Newton examines the practical question of how the resistance of the air can be allowed for in pendulum experiments. He has in view here the experimental verification, with as great accuracy as was possible, of his principle that the gravity of a body at a particular place is proportional to its mass. He proceeds to speculate on the nature of the resistance which a fluid opposes to the motion of a body through it. Since this must be due to the momentum communicated to the medium, the law of the square of the velocity is inferred, more or less approximately. Neglecting viscosity, he treats rigorously the case of a medium composed of discrete and inert particles. When he proceeds to continuous fluids such as water, the reasoning is confessedly of a more general character. Incidentally, there occurs a remarkable application of the principle of dynamical similarity, a long anticipation of Fourier.

The remaining propositions deal with hydrostatics and wave-motion. Under the former head we find the law of pressure in an isothermal atmosphere surrounding a globe. Another result, remarkable for the originality of the argument employed, is that if Boyle's law were a statical consequence of repulsive forces between the particles of air, the law of force must be that of the inverse distance. The propositions on waves no doubt owe their insertion to the difficulty which Newton felt in accepting the undulatory theory of light. An ingenious if imperfect attempt is made on the theory of deep-water waves. This is followed by the well-known demonstration of the possibility of sound-waves of harmonic type, and the calculation of their speed of propagation, on the basis of Boyle's law.

From the physical speculations which had attracted some of his contemporaries, Newton carefully refrains. He insists repeatedly that when he refers to accelerating forces or moving forces or centripetal forces he is speaking mathematically, and puts forward no hypothesis as to their ultimate origin. "*Nam virium causas et sedes physicas iam non expendo.*"

This rapid survey necessarily omits much that is interesting. But it should at any rate indicate the enormous debt which the science of Mechanics owes to Newton, for the originality of his conceptions, and for the resolute way in which problem after problem is attacked and made to yield results of interest and beauty as well as of far-reaching importance. It is true, as he indicates, that the fundamental principles had already come to be recognised in a measure, and applied to a few isolated problems. They were now finally vindicated by the host of new and unquestioned results which were shown to flow from them.

Two of the fundamental conceptions were especially due to Newton himself. One is the distinction between gravity and inertia. The other is that of the universal validity of the law of action and reaction, with, in particular, the deduction that the total momentum of a system is unaffected by internal forces. Some appreciation of this is to be found in the work of Huyghens and others on collision, but the generalisation belongs to Newton. A wide extension of the principle is given near the end of the long scholium which he devotes to the explanation and illustration of his third law. It is to be observed that apart from this extension the Newtonian scheme is strictly one of Particle Dynamics, and does not furnish the means of studying in detail the relative motions of the different parts of a solid or fluid of finite dimensions. For this, some additional principle or hypothesis is required, such as was introduced at a later date by d'Alembert and others. It was long overlooked that a sufficient assumption for the purpose is given at the end of the aforesaid scholium. Not only do we find here what is possibly the first general enunciation of the principle of virtual velocities, as distinguished from the partial exemplifications by Galileo and others, but also by the explicit inclusion of accelerating forces the principle becomes identical, when translated into analytical language, with the "variational equation" which Lagrange long afterwards adopted as the basis of his exposition of Dynamics. With this supreme example of the insight of the master this review may fitly close.

## Newton's Work in Physics.[1]

By Sir J. J. Thomson, O.M., F.R.S.

THE middle of the seventeenth century, when Newton was born, was remarkable for an outburst in natural philosophy akin to that in literature at the Renaissance. New ideas, new inventions, new discoveries were coming forward both in England and on the Continent. The Royal Society had just been formed to discuss scientific questions and to witness scientific experiments. The soil had been prepared by the work of giants like Descartes, Hooke, Boyle, and Huyghens. To Descartes more than to any one else was this outburst of interest due. It was he who invented the ether; by his theory of vortices he had supplied a consistent and comprehensive theory in which it was conceivable all physical phenomena might find their place and explanation. His theory clothed with romance and fascination the dry bones of science; to see how it fitted into the theory made each new discovery furnish a most fascinating intellectual problem. Later there was much contention between Cartesians and Newtonians, but let us who are Newtonians acknowledge the debt science owes to Descartes, the man whose "Treatise on Geometry" attracted Newton to mathematics and, he said, gave a vaster idea of geometry and the use of algebra than it was possible for him to express or for one who had not read it to imagine.

I am to speak to-day about Newton's work in physics. I will begin by pointing out what is perhaps generally not realised, his skill in the practical as well as the theoretical part of physics. He was an excellent manipulator and experimenter, and liked using his hands. Almost the only recreation in which he seems to have indulged as a boy was to make little work-boxes and trinket cases for his girl friends. He made the first reflecting telescope with his own hands. In the "Optics" he writes about the methods of grinding lenses with a gusto and wealth of detail which show what an old hand he is at the game. When, as in one of the Queries, he discusses chemical questions, he revels in details known only to those who have spent long hours in a chemical laboratory. Mr. Humphrey Newton, who acted as his amanuensis and assistant from 1683 until 1689, says: "At the spring and fall of the leaf he used to employ about six weeks in the laboratory, the fire scarcely going out either night or day, he sitting up one night and I another till he had finished his chemical experiments, in the performance of which he was the most accurate, strict, exact." At this time Newton was working at the transmutation of metals, and I think it exceedingly probable that he had spent more time over this subject than in writing the "Principia."

Newton's great discovery—the splitting up of white light into a spectrum of different colours—was led up to by his seeking for a cause for the bad definition of the refracting telescopes of the time. This was generally attributed to what is called spherical aberration, the rays which pass through the outer parts of the lens not being brought to the same focus as those which pass through the centre; indeed, Descartes had worked out elaborate shapes for lenses in order to remedy this. Newton seems to have convinced himself that there was more in it than this, and was thus led to make his famous experiment. He passed a narrow beam of white light through a prism and found that what had been narrow and white before falling on the prism, after passing through it was spread out into a broad band showing all the colours of the rainbow—red at one end, blue at the other, and between them a gradation of different colours which he divided into seven classes, red, orange, yellow, green, blue, indigo, and violet. He took a narrow beam from this coloured band and let it pass through another prism, and found that it behaved quite differently from the original white beam, and that it was not split up into a broader beam. A narrow beam of red light before falling on the second prism was a narrow beam of red light after passing through it. Again starting with a narrow beam of white light he split it up into a spectrum; he then sent the spectrum through another prism like unto the first but turned the other way up, and reproduced the narrow band of white light again. He first "untwisted the shining robe of day" and then put it together again.

The chapters in the "Optics" where these experiments are described give one, I think, an impression of intellectual power almost unparalleled in the history of physics. Every experiment—nay, almost every sentence—clears up some essential point, and on reading them again a few days ago to refresh my memory, I was even more impressed than I had ever been before, and re-echo the advice of the late Lord Rayleigh, that "*every* student of

[1] Address delivered in King's School, Grantham, on Mar. 19, at the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

physics should read the earlier parts of Newton's 'Optics.'"

It is one of the ironies of science that the outcome of a successful attempt to get at the root of the reason for the imperfections of the refracting telescope should have had the effect of delaying the improvement of that instrument for the best part of a century. Newton diagnosed the disease, but came to the conclusion that it was incurable. As the defect was due to the different coloured rays being differently bent when they passed through a lens, to effect a cure it was necessary to use two lenses, one bending the rays in one direction, the other in the opposite, and to try to adjust the shape and materials of these lenses so that the difference in the bending of, say, the red rays is the same as that of the blue. If the system is to act as a telescope there must be some bending of each of the rays, and this was the difficulty. Newton came to the conclusion that the difference in bending of two rays was always in the same proportion to the average bending whatever the material of the lens. From this it follows that when the difference in bending vanishes the whole bending does so too, so that the system ceases to act as a telescope.

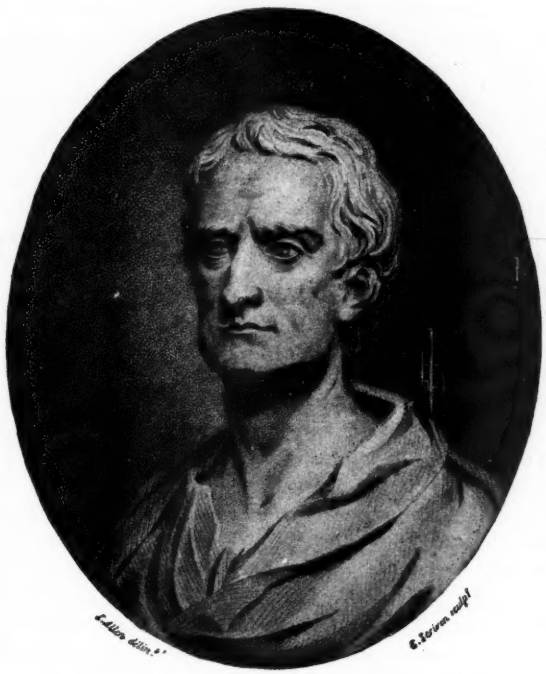

FIG. 7.—Bust of Newton in the Royal Observatory, Greenwich.

I think there were two reasons why Newton came to this conclusion: one was that the prisms he used were either prisms of light glass, or hollow prisms filled with water. He says that he used salt water in these prisms, and it is exceedingly probable that the dispersion of the light glass was almost identical with that of the salt water. There was, however, another reason which in my opinion was the one that influenced him most. Most of us, I think, on looking at the spectrum, would suppose that the number of colours to be distinguished is rather a question of the number of names our language supplies for different colours than of anything else. I do not think Newton held that opinion. He seems to have regarded the different colours—red, orange, yellow, green, blue, indigo, and violet as, so to speak, different genera and the light inside these colours as different species; he therefore attaches great importance to the places where one of these colours begins and the other ends. As "his own eyes are not very critical in distinguishing colour," he got a friend to measure the length of the different coloured spaces.

Now, unfortunately, the divisions between the different colours given by these measurements turned out to be in the same proportion as a string is divided between the end and the middle to give the divisions of the octave on the diatonic scale. Thus if the length of the string giving the lowest note is 1, and the length of the spectrum $\frac{1}{2}$, the length of the string giving the octave will be at the end of the violet, the length of the string giving the note next to the octave will be at the junction of the indigo and violet, the next at the junction of the indigo and blue, and so on. I think Newton was profoundly influenced by this view: he returns to it in his work on the colours of thin plates, and shows that the thicknesses of the plates which give the junction colours are proportional to the cube roots of the squares of the length of these chords on the diatonic scale. As on this view the widths of the different colours are always in fixed numerical ratios, the spectrum given by one substance will be exactly similar to that given by another; this means that the dispersion of all substances is the same and that an achromatic combination is impossible. It was, I think, the siren's song of these harmonics that lured Newton to this false conclusion. We must remember that Newton had no suspicion that the spectrum as he saw it, beginning at the red and ending at the violet, was not a complete entity, or that there was anything on either side of it; we know now

that the visible spectrum is but a more or less arbitrary piece of a much larger structure.

It was, too, a very prevalent belief that these harmonics were the key to many of the mysteries of Nature. Kepler, for example, spent many years' work trying to express the motion of the different planets in terms of these harmonics. Holding these views, Newton came to the conclusion that the chance of making a refracting telescope was desperate; he abandoned what he called his glass work and devoted himself to developing the reflecting telescope. He was the first to construct such a telescope, though Gregory had provided him with a design for one on somewhat different lines. It is a remarkable illustration of Newton's influence on the science of his time, and for long after, that his error about achromatism was not corrected for more than fifty years, and that when at last this was done, it was due to the independent researches of two practical men, one a country gentleman and the other an instrument maker, and not to a philosopher attached to any seat of learning.

Newton next applied himself to the study of the colours of thin films, such as soap bubbles, and thin pieces of mica. These had previously been studied by Hooke and Boyle, and Hooke had published a theory of them in a paper which is one of the most remarkable in the history of optics; in it he foreshadows the principle of interference, and Young, the discoverer of this principle a century later, said that a knowledge of this paper would have materially hastened the discovery. The observations of Hooke and Boyle were entirely qualitative. Newton, as was his wont, reduced everything to definite numbers. His extraordinary powers of observation, and his genius for reducing to a few fundamental principles a mass of confused and perplexing phenomena, were never more conspicuous than in this investigation. The subject, which before he began his work had been but a medley of facts without any apparent connexion, was reduced by him to law and order; so much so that even now there are few better or clearer accounts of the fundamental phenomena, apart from their explanation, than that given in the "Optics." Surprisingly few of the great number of effects shown by these thin films escaped Newton's notice. He discovered the law connecting the thickness of the film with the colour it shows; he gives us the first measure of a quantity akin to the one we call now the wave-length of the light. He supposes that a ray of light as it travels through space alternates between two moods; when in one of these moods it falls on a surface it is reflected, when in the other it is transmitted. Each mood lasts while the ray travels a certain distance, and the ray is supposed to be always in one or other of the moods. He calls these moods 'fits' of easy transmission and reflection, and the quantity he measured is the space passed over by light on the duration of one of these fits. He deduced from his work a scale of colour by which a colour was classified by the thickness of the plate which gave rise to it. His great powers of observation were shown in the discovery of what are known as the colours of thick plates, which generally require some finding even when one knows where to look for them. He showed, too, that solar and lunar haloes were due to the presence of minute drops of water all of the same size.

Newton's experiments on thin plates so impressed him with their possibilities for the production of colours that he brought forward a theory of colour in which he supposes that the colours of all natural bodies, even coloured solutions such as wine, arise in this way. He supposes that the smallest particles of bodies are transparent and would be colourless if alone. When, however, they congregate together, as in solids and liquids and to some extent in gases, their parts are separated by interstices, and a rough description of his theory is that the colour of a body is that of a thin plate the thickness of which is equal to the interstice. Newton, though he had an accurate idea of the scale of the structure of light, very much overestimated the coarseness of the structure of matter. He says in his "Optics" that if we could make microscopes to magnify some 5000 times, we could probably detect these interstices. He thought that the interstices were of the same order as the length of a 'fit' of easy transmission or reflection, whereas we know that they are less than 1/1000 of that distance, much too small to be of any use for Newton's theory.

Another ingenious application of the colour of thin plates was his explanation of the blue colour of the sky. He supposes that it is due to minute bubbles of water in the air, and that the bubbles are thin enough to make the blue predominate. This theory lasted until comparatively recently, when the late Lord Rayleigh showed that a quite distinct though in some respects analogous effect, the scattering of light by small particles in the air, gave an explanation more in accordance with the facts.

I now turn to the question of Newton's views as to the nature of light. Newton was always exceedingly careful not to tie himself down to any precise specification of the structure of light. He would not, I think, have accepted as a correct

representation of his views the theory that was fathered upon him by his successors, that light consisted of small material particles and nothing else. We have in the letters to Hooke and Boyle a record of the ideas about light which were passing through his mind when he was busiest with his optical researches, and we find that then the ether was an integral part of his conception of light. He says: "Were I to propound an hypothesis it should be this, that light is something capable of exciting vibrations in the ether." He gives, however, his reasons for thinking that there must be something besides these vibrations, and gives a long list of alternatives.

"They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies. . . . But they that like not this may suppose light any other corporeal emanation, or any impulse or motion of any other medium or etherial spirit diffused through the main body of aether or what else they may imagine proper for their purpose. To avoid dispute and make this hypothesis general, let every man here take his fancy, only whatever light be I suppose it consists of rays differing from one another in contingent circumstances as bigness from vigour."

Again, in his letter referring to Hooke's undulating theory he says: "The hypothesis of light being a body, had I propounded it, has a much greater affinity with the objector's own hypothesis than he seems to be aware of, the vibrations of the aether being as useful and necessary in this as in his."

In the "Optics," published thirty years later, which begins: "My design in this book is not to explain the properties of Light by Hypothesis, but to propose and prove them by reason and experiment," the ether is not introduced into the body of the book. The idea of 'fits' of easy transmission and reflection is sufficient for his purpose; he just postulates the existence of these 'fits,' saying: "I content myself with the bare discovery that the rays of light are by some cause or other alternately disposed to be reflected or refracted through many vicissitudes." But if the ether is banished from the three books of the "Optics" it appears in full vigour in Query 29. Newton says:

"Are not the rays of light very small bodies emitted from shining substances? Nothing more is requisite for putting the Rays of Light into Fits of easy Reflection and easy Transmission than that they be small bodies which by their attractive power or some other Force, stir up Vibrations in what they act upon, which Vibrations being swifter than the Rays overtake them successively and agitate them so as by turns to increase and decrease their velocities and thereby put them into those Fits."

Thus Newton regarded light as possessing a dual structure, one part of which was the small corpuscle, the other the vibrations which surround it. One very important feature of Newton's theory of light, and one which differentiates it very sharply from the undulatory theory, is that on Newton's theory the structure of light is essentially atomic; it is made up of discrete and definite parts. He says in his first definition, "by the rays of light I understand its least parts." He regards light as made up of those parts which travel through space unchanged; the light coming to us from a star is made up of particles of exactly the same kind as those in the light as the star itself; the only difference is that the particles get more and more widely separated, as the distance from the star increases.

Let me illustrate the difference between this result and that which obtains on the undulatory theory by the consideration of the following case. Suppose we have a battery of guns in action. The guns emit both shot and waves of sound; as we go farther from the guns our chance of being hit by the shot gets smaller, but if we are not so far away that the shot has lost speed, the effect when we are hit is just as bad as if we were nearer to the guns; the effect of increasing the distance is to diminish the number of casualties without changing their character. Now consider the sound waves. Let us call the striking of these waves against our ears a casualty; also when we go to a greater distance the chance of these continuous waves hitting us will be just as great as when we are nearer in, but the noise will fall away quickly as the distance increases. In this case the number of casualties will not diminish with the distance but their character will change. This is a fundamental difference between a corpuscular and an undulatory theory.

In recent years great attention has been paid to the electrical effects produced by light; one of these is the emission of electrons from a metal surface when light falls upon it. The number and velocity of these electrons can be measured with considerable accuracy. It has been found that as the distance of the metal from the source of light increases, the number of electrons emitted decreases; that is, the number of casualties diminishes, but those electrons which are emitted are moving just as fast as when the metal was close to the source of light: that is, the character

of the casualties is not changed. This is but one of many of the electrical effects produced by light which show the same characteristic. In fact, all these effects indicate that the structure of light must be atomic rather than continuous. If we confine ourselves to the corpuscles, though we might explain the electrical effects we could not explain the optical phenomena of interference; but we must remember that Newton in his confidential moods never contemplates the corpuscle as being the sole constituent of his units. These were always accompanied by vibrations in the ether, and the effect of these as well as the corpuscles must be taken into account.

At the end of the "Optics" come the Queries. In these Newton abandons the severe, almost Euclidean, style of the earlier part of the book; he flings away his policy of "hypotheses non fingo"; he makes up for lost time. The suggestions he makes are extraordinarily acute and suggestive. Here is one of them.

"Are not gross bodies and light convertible into one another and may not Bodies receive much of their activity from the particles of Light which enter into their composition. The changing of bodies into Light and Light into Bodies is very conformable to the course of Nature which seems delighted with transmutations."

In another Query Newton connects the abnormal refracting powers of some substances with their chemical nature, a subject which is now of great importance. The connexion he suggests is that since these bodies are so affected by light, their chemical nature is such as to make them readily take fire and emit light for themselves. This is the first and most daring of reciprocal relations of the type we are now familiar with in thermodynamics.

Newton suggests that the ether in itself is atomic and that the atoms may not all be of one size. He calculates from the rise of liquid between two glass plates the force exerted by the attraction of the particles of the glass on water at a distance of $\frac{3}{8}$ of one hundred thousandth part of an inch, that is, about one millionth of a centimetre, and finds it sufficient to hold up a cylinder of water two or three furlongs in length. He has extraordinary clear and definite ideas about chemical combination much in advance of anything which appeared for more than a century afterwards.

I have confined myself to Newton's work on optics. I have not time to do more than recall that he was the first to give the theory of the propagation of waves of sound. His work in physics is but a part, and perhaps not the most important part, of his scientific work, but if it stood alone his would still be one of the great names in science.

## Newton's Work in Pure Mathematics.[1]

By Prof. L. J. Mordell, F.R.S.

WHEN we consider the numerous and wonderful developments of mathematics since the beginning of the last century, it is very difficult for us to appreciate the state of mathematics just before the rise of Newton. The period was a critical one in the history of this science. All the signs pointed to a great awakening, and the world was ripe for important and far-reaching discoveries. This was the time of the last days of Fermat (1601–1665) and of Descartes (1596–1650), who had both initiated epoch-making discoveries. From the modern point of view, what little was known related to geometry, trigonometry, algebra, and the theory of equations. Their fundamental principles had been laid down roughly in the form in which they are now familiar to elementary students. General theorems were extremely scarce. Each new differential property of a curve, each new expansion of a function of $x$, required new methods. Mathematics was chiefly a collection of isolated theorems and examples.

Many distinguished mathematicians have shown unmistakable signs of mathematical genius at an early age. Newton, however, knew no mathematics when he entered Trinity College in 1661 at nineteen years of age. He was introduced to mathematics in his first term by the purchase of a book on astrology, which he could not understand because of the references to geometry and trigonometry. He then started to study Euclid's geometry, which he found very easy and almost obvious. He followed with a book on arithmetic and Descarte's "Géométrie," which was difficult enough to interest him. As an undergraduate he also read the works of Vieta, Van Schooten, and Wallis.

Newton's original investigations were commenced early in his career. In a manuscript of his written in 1665, the year in which he took his B.A. degree, there is the earliest documentary proof of his invention of the fluxional calculus, that is, what is

[1] Address delivered in King's School, Grantham, on Mar. 19, at the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

now called the infinitesimal calculus. He had worked this out fairly completely and had applied it to finding tangents, radii of curvature, etc., of curves. It also led him to the discovery of the binomial theorem.

It is given to very few people, even in their later years when their powers have fully matured, to make so fundamental, so brilliant, so epoch-making a discovery as the invention of the calculus. Thus it is all the more marvellous that Newton should have done so in his early manhood after so short a period of study. His discovery represented the second stage in the emancipation of mathematical ideas from the bondage in which they seem to have been held since the time of the ancient Greeks. The first stage was due to Descartes and his system of analytical geometry.

Newton's discovery meant in those days even more than Cauchy's theory of the complex variable meant in his. It seemed to unlock the gates guarding the storehouse of mathematical treasures; to lay the mathematical world at the feet of Newton and his followers; to open up new paths, new regions that would require years to develop.

It is uncertain how far Newton developed his calculus or what were the dates of his discoveries, as he was very reluctant to publish his results. Many of his most important discoveries had their origin in problems arising from the calculus. Others, especially in geometry, arose from his endeavour to put the proofs of theorems of applied mathematics in a form free from the calculus, which appeared to be greatly distrusted for years after its invention because of the logical difficulties encountered by the mathematicians and philosophers who took up its study.

The question of integration was one of the most fertile sources of Newton's results. Thus in 1676, in letters to Oldenburg, the secretary of the Royal Society, who had been corresponding with Leibniz about some of the latter's results, Newton describes some of his own work and some of his methods of discovery, and in particular of the binomial theorem.

The binomial theorem was discovered in finding the integral of $(1-x^2)^{\frac{1}{2}}$. Newton noted that

$$\int(1-x^2)dx = x - \frac{x^3}{3},$$

$$\int(1-x^2)^2dx = x - \frac{2x^3}{3} + \frac{x^5}{5},$$

$$\int(1-x^2)^3dx = x - \frac{3x^3}{3} + \frac{3x^5}{5} - \frac{x^7}{7}.$$

He studied the forms of the coefficients, and induction suggested to him the result

$$\int(1-x^2)^{\frac{1}{2}}dx = x - \frac{\frac{1}{2}}{3}x^3 + \frac{\frac{1}{2}\cdot\frac{1}{2}-1}{2!}\frac{x^5}{5} - \frac{\frac{1}{2}\cdot\frac{1}{2}-1\cdot\frac{1}{2}-2}{3!}\frac{x^7}{7}\cdots.$$

He then deduced the binomial expansion for $(1-x^2)^{\frac{1}{2}}$, which he verified by squaring and showing that the result reduced to $1-x^2$. He then gave without proof the expansion of $(a+b)^n$ as an infinite series. It was only after many years that a satisfactory proof was obtained by Abel. Newton realised that infinite series should be convergent, but did not know any tests for convergency. The present-day student would consider it an easy matter to find inductively the well-known expansion of $(1+x)^n$. It comes rather as a shock to realise that the winning of the simple, elementary mathematics of to-day required mighty geniuses at some time or other.

Newton also gave in this letter the expansion of $\sin^{-1}x$. He deduced from this the expansion of $\sin x$ by the method of reversion of series, the earliest example of this process. History was to repeat itself many years afterward when the expansions of elliptic integrals were to precede those of elliptic functions. He also found the length of an arc of an ellipse by infinite series. He evaluated the binomial integral

$$\int x^m(b+cx^n)^p dx,$$

which can be integrated in finite terms if $(m+1)/n$ is a positive integer, and which he thought, erroneously, could not be done otherwise without infinite series. In later years, he also solved the problems of orthogonal trajectories and of the brachistochrone. He solved differential equations by the use of infinite series, and, as he says, any differential equation can be integrated in this way. But this of course does not end the matter.

Very little need be said about the discussion which raged for so many years as to whether Newton or Leibniz deserved priority in the invention of the calculus. It is now considered that they were independent discoverers. This is not really surprising when the work and interests of the mathematicians of their era are considered. The history of mathematics abounds with similar examples. The theory of elliptic functions was discovered independently by Abel and Jacobi, and it was only many years after the death of Gauss that his notebooks revealed that he had anticipated

both of the others, but had not published his work.

Newton lectured on algebra and the theory of equations during the years 1673–1683, and many theorems on these subjects are now associated with his name. The reversion of series has already been mentioned. Another very important theorem, now fundamental in the theory of algebraic functions of a variable, is referred to as Newton's parallelogram. It discusses the shape of a curve with a multiple point at the origin or at infinity and is equivalent to finding from a given relation $F(x, y)=0$ the initial terms in the expansion

$$y=Ax^{\frac{p}{q}}+Bx^{\frac{r}{s}}+\ldots.$$

The calculus also led him to the method of interpolation. By this he approximated to the shape of a curve by drawing a higher parabolic curve

$$y=a+bx+cx^2+dx^3+\ldots$$

through a number of points on the given curve. He was thus enabled to find the area approximately by easy integration.

Newton left his impress particularly upon the theory of equations. He proved that imaginary roots occurred in pairs. It may be recalled that it was only in the nineteenth century that the importance of imaginary numbers was appreciated and fully understood. His formula for the lower limit to the number of imaginary roots in an equation was given without either proof or any indication of the method by which he discovered it. It remained for Sylvester to prove in 1865. Newton laid the foundation for the theory of the symmetrical function of the roots of an equation by giving his well-known formula for the sum of the $n$th powers of the roots in terms of the coefficients of the equation.

Newton's name has been given to a method for finding approximately the root of an equation. In the form in which it is now used, namely, that if $a$ is an approximation to a root of the equation $f(x)=0$, then

$$a-\frac{f(a)}{f'(a)}$$

is in general a better approximation, the theorem is not due to Newton but to Raphson.

Newton's method as exemplified by his famous cubic

$$y^3-2y-5=0$$

was to put

$$y=2+p,$$

so that the equation becomes

$$p^3+6p^2+10p-1=0.$$

Neglecting the higher powers of $p$, he gets $10p=1$. He then puts

$$p=0{\cdot}1+q$$

in the equation for $p$ and gets

$$q^3+6{\cdot}3q^3+11{\cdot}23q+0{\cdot}061=0.$$

The approximate value of $q$ is given by

$$11{\cdot}23q+0{\cdot}061=0.$$

He then puts

$$q=-0{\cdot}0054+r,$$

etc.

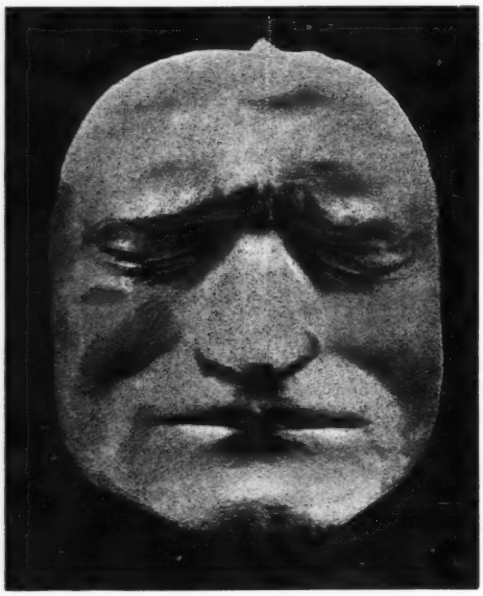

Fig. 8.—Cast of Newton's face, in the possession of the Royal Society, London.

From Green's "Short History of the English People."

We come finally to the geometrical work of Newton. He was unequalled by modern writers in his power in the use of the methods of classical geometry, though he discovered no new principles. The geometrical solution of the problem of Pappus, to find the locus of a point such that the rectangle contained by its distances from two given lines shall be in a given ratio to the rectangle contained by its distances from two other given lines, had baffled the efforts of geometers since the time of Apollonius. Newton apparently gave without much difficulty an elegant proof that the locus was a conic.

Equally skilful was Newton in the application of analytical geometry to the theory of curves. He distinguished between algebraic and transcendental curves, and showed that the former were cut by a straight line in a number of points given by the degree of the equation, and the latter in an infinite number of points. This, of course, is a fundamental theorem in geometry.

Newton investigated the properties of cubic curves analogous to those of conics such as asymptotes and diameters. He then proceeded

to reduce the equation of a cubic to four simple canonical forms. Each of these was discussed in detail, and its double points, ovals, etc., were considered. He found seventy-two of the possible seventy-eight forms that a cubic may take. Finally, he stated without proof that all cubic curves could be derived by projection from the canonical form

$$y^2 = ax^3 + bx^2 + cx + d,$$

a theorem which remained unproved until 1731. Apart perhaps from some of his geometrical investigations, Newton's pure mathematics was dominated by his invention of the calculus and his efforts to solve the problems of Nature. No one has surpassed him in combining theory and its application. No one appreciated him more than the greatest of the great mathematicians.

## Newton's Work in Optics.

By Sir Richard Glazebrook, K.C.B., For. Sec. R.S.

NEWTON entered Trinity College, Cambridge, in 1661 shortly before his nineteenth birthday, and proceeded to his B.A. degree in 1665. On Oct. 1, 1667, he became a fellow of the College, and in 1669 was chosen as Lucasian professor of mathematics in succession to Barrow, whose optical lectures he had helped to edit during the previous year. His own interest in optics began at an early date; while still an undergraduate he had written about haloes. In 1666 he "procured a triangular glass prism to try the celebrated phenomena of colours." His first telescope was constructed in 1668 and his second telescope sent to the Royal Society in 1671. The following year (Jan. 11, 1671, old style) he was elected a fellow of the Royal Society, and his letter containing his new theory of light and colours was read on Feb. 6, 1672. Hooke, who with Boyle and the Bishop of Salisbury was asked to report on the paper, wrote a critical report, claiming that Newton's results were contained in his own "Micrographia" published in 1664. Criticisms were also published by Linus of Liège and Lucas.

Newton was admitted as a fellow of the Royal Society in 1675, and shortly afterwards sent to the Society a paper on the colours of thin plates. Hooke was again a critic. The main part of the work, he said, "was contained in the 'Micrographia,' which Mr. Newton had only carried further in some particulars."

Some correspondence followed, and Newton in his final letter gave Hooke full credit for his work. "You," he wrote, "have added much several ways, and specially in considering the colours of thin plates. If I have seen farther it is by standing on the shoulders of giants."

It was not until some thirty years later (1704) that the first edition of the "Optics" appeared. A second edition with additions—"A Treatise," it is called, "of the Reflexions, Refractions, Inflexions, and Colours of Light"—was printed by the printers to the Royal Society in 1718, fifty years after the construction of his first telescope, and contains, in a series of Queries of very real interest, his latest views on optics and allied subjects.

But before dealing with the questions raised in these, some reference to the book itself is called for. The difficulties which followed the reading of his papers in 1675 were the main cause of the delay in publication. "To avoid being engaged in Disputes about these Matters," he writes, "I have hitherto delayed the printing, and should still have delayed it had not the Importunity of Friends prevailed upon me." The treatise is divided into three books. The first deals with the reflexion, refraction, and dispersion of light; the second with the colours of thin and thick plates; and the third with diffraction, and in the Queries with double refraction and the theory of light.

The first book opens with a statement of known propositions as a series of definitions and axioms. "For what hath hitherto been generally agreed on I content myself to assume under the Notion of Principles. . . . And this may suffice for an Introduction to Readers of quick Wit and good Understanding not yet versed in Optics, although those who are already acquainted with this Science and have handled Glasses will more readily apprehend what followeth," and then we come to Prop. I. Theor. I. "Lights which differ in Colour differ also in Degrees of Refrangibility." From this we pass by a series of experiments most beautifully designed, most clearly described, to an investigation of the chromatic aberration of lenses with an account of the defects of refracting telescopes, incurable, as Newton thought he had proved it to be by these experiments, and the construction of his own reflecting instrument. "Seeing, therefore," he writes, "that the Improvement of Telescopes of given lengths by Refractions is desperate, I contrived therefore a Perspective by Reflexion, using instead of an Object-glass a concave Metal." The mirrors of the first two telescopes he made and polished himself.

"By many trials I learnt the way of polishing till I made those two reflecting Perspectives. . . .

For this Art of polishing will be better learned by repeated Practice than by my Description."

"When I made these an Artist in London undertook to imitate it, but using another way of polishing them than I did he fell much short of what I had attained to, as I afterwards understood by discoursing the under Workman he had employed."

Newton was mistaken, as we now know. Fig. 9—Fig. 26, Plate V., of the first book of the "Optics"—illustrates the crucial experiment (Exp. 15, Bk. I. Part I.). In the figure PT is the position of the spectrum formed on a vertical screen by a prism with its edge horizontal and its angle downwards. The plane of the screen is parallel to the edge of

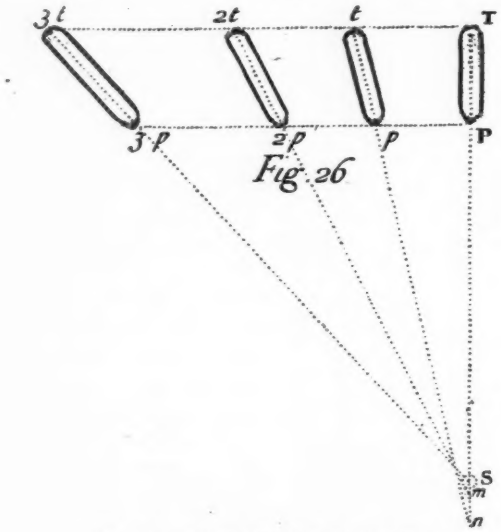


FIG. 9.—Reproduced from Newton's "Optics."

the prism. The light forming this spectrum is intercepted by a second prism placed close behind the first, but with its edge vertical; the spectrum is displaced into the position *pt*. T*t* and P*p* measure the tangents of the deviation of the violet and red ends respectively, while their difference depends on the dispersion. 2*t*2*p*, and 3*t*3*p* represent spectra formed similarly by other prisms placed in turn in the position of the first vertical prisms. S is the position of the spot formed on the screen by the direct light of the sun, painting "his round white image when the prisms were taken away."

Newton observed that when the angle of the prism forming the image 3*p*3*t* was equal to that of the first prism, the axis of the spectrum 3*t*3*p* met that of the original spectrum TP at the centre of the sun's image S, while the produced axes of the spectra *tp* or 2*t*2*p* formed by prisms of less angle cut the axis of TP in the points *m*, *n* a little beyond the centre of the image S. From the figure he obtained the values of the tangents of the deviations of the violet and red rays in each case

"from whence the Proportions of the Sines being derived, they come out equal so far as by viewing the Spectrums and using some mathematical Reasoning I could estimate";

or, in other words, if we denote by $\alpha$, $\beta$, the deviations of two rays (violet and red, say) produced by a prism, then the ratio $\sin\alpha/\sin\beta$ is independent of the prism.

It is, however, not clear from the description that the prisms were of different material. With modern glasses of high or moderate dispersion the axes of the spectra would not be straight, but curved, and would not appear to meet near S.

Newton refers to the different angles of the several prisms, and continues:

"But for want of solid Glass Prisms with Angles of convenient bignesses, there may be Vessels made of polished plates of Glass cemented together in the form of Prisms and filled with Water."

It is at any rate possible, or perhaps rather probable, that he proved the proposition only in the case of prisms of the same material, and in any case it is not likely that he used prisms of widely different optical properties, even though they may not all have been bought at Stourbridge Fair, to try the celebrated phenomena of colours. And so he concludes:

"But by reason of this different Refrangibility I do not see any other means of improving Telescopes by Refractions alone than that of increasing their lengths, for which end the late Contrivance of Hugenius seems well accommodated."

But to pass on; other propositions of the book deal with the nature of white light and the composition of colours.

"Colours," it is shown, "may be produced by Composition which shall be like to the Colours of Homogenial Light as to the appearance of Colour, but not as to Immutability of Colour and Constitution of Light";

and again:

"Whiteness, and all grey Colours between white and black, may be compounded of Colours, and the whiteness of the Sun's Light is compounded of all the primary Colours mixed in a due Proportion."

In another proposition it is shown that if light be due to the motion of corpuscles moving in the direction of the ray, but subject, when in the neighbourhood of the surface of separation of two different media, to a force normal to the surface, and if $v$, $v'$ be the velocities in the two media, $\phi$, $\phi'$ the angles

of incidence and refraction, then $\sin\phi/\sin\phi' = v'/v$. Thus if $\phi$ is $> \phi'$, then $v'$ is $> v$, or, of the two media, the velocity is greater in the more dense, contrary to what we now know to be true.

So far Newton has been dealing with geometrical optics. Book II. introduces us to "Observations concerning the Reflexions, Refractions, and Colours of thin transparent Bodies"—to Newton's rings and the theory of light he devised to account for his observations.

Hooke in 1665, and Boyle two years earlier, had observed the colours shown by thin sheets of mica in both transmitted and reflected light, and Hooke ("Micrographia," p. 47) had described how to produce these colours by placing two lenses in contact and reflecting light from the thin film of air between them. Newton, after a brief reference to the observations of others, describes his own experiments. He found the squares of the radii of the bright rings to be in the arithmetical progression of the odd numbers 1, 3, 5, . . . and those of the dark rings in the arithmetical progression of the even numbers 0, 2, 4, 6, . . .

A knowledge of the radii of curvature of the lenses and of the radii of the rings enabled him to calculate the thicknesses of the air film for light and dark rings. These he found to be, in inches, 1/178000, 3/178000, . . . and 0, 2/178000, 4/178000 . . . respectively.

Having now obtained his facts, Newton attempts to explain them :

"Every Ray of Light," he writes (Bk. II. Pt. iii. Prop. xii.), "in its passage through any refracting Surface is put into a certain transient Constitution or State which in the progress of the Ray returns at equal Intervals and disposes the Ray at every return to be easily transmitted through the next refracting Surface, and between the returns to be easily reflected by it." And then follows a definition : "The returns of the disposition of any Ray to be reflected I will call its Fits of easy Reflexion, and those of its disposition to be transmitted its Fits of easy Transmission, and the space it passes between every return and the next return the Interval of its Fits."

He has no theory to account for this, but writes :

"What kind of action or disposition this is ; whether it consists in a circulating or a vibrating Motion of the Ray or of the Medium or something else, I do not enquire."

He postulates (Query 17) a refracting or reflecting medium in which waves of vibration or tremors are excited at a point in which a ray of light is incident, "and continue to arise there and be propagated from thence as long as they continue to do so." While (Query 18) he writes, after describing an experiment which shows that heat can reach a thermometer when suspended in a vacuum :

"Is not the Heat of the warm Room conveyed through the *Vacuum* by the Vibrations of a much subtiler Medium than Air, which after the Air was drawn out remained in the *Vacuum* ? And is not this Medium the same with that Medium by which Light is refracted and reflected, and by whose Vibrations Light communicates Heat to Bodies and is put into Fits of easy Reflection and easy Transmission ? And do not the Vibrations of this Medium in hot Bodies contribute to the intenseness and duration of their Heat, and do not Hot Bodies communicate their Heat to contiguous cold ones ? And is not this Medium exceedingly more rare and subtile than the Air, and exceedingly more elastick and active ? And doth it not readily pervade all Bodies ? And is it not (by its elastick force) expanded through all the Heavens ?"

And again, Query 19 :

"Doth not the Refraction of Light proceed from the different density of this Ethereal Medium in different places, the Light receding always from the denser parts of the Medium ? And is not the density thereof greater in free and open Spaces void of Air and other grosser Bodies than within the Pores of Water, Glass, Crystal, Gems, and other compact Bodies ?"

These queries appeared in 1718. In the text of the "Optics" (1704), Newton was more cautious, for in Prop. xii., after his inquiry as to the action or disposition which causes the alternate fits of reflexion and transmission, he permits "those who are averse from assenting to any new Discoveries but such as they can explain by an Hypothesis" to "suppose for the present" that "the Rays of Light by impinging on any refracting or reflecting Surface, excite vibrations in the reflecting or refracting Medium or Substance."

We will return below to the question of why this complexity was necessary.—A stream of corpuscles constituting a beam of light, accompanied in their path by waves in an elastic medium travelling with a velocity exceeding that of the corpuscles themselves, overtaking them and jostling them into alternate fits of reflexions and transmission, a theory so complex that in the words of Verdet :

"Pour renverser ce pénible échafaudage d'hypothèses indépendantes les unes et les autres il suffit presque de le regarder en face et de chercher à le comprendre."

Meanwhile some brief account must be given of the Third Book of Optics : "Observations concerning the Inflexions of the Rays of Light and the Colours made thereby." It deals with Grimaldi's observation of the colours of bands

seen on either side of the shadow of a hair illuminated by light from a small hole—the diffraction of light.

Newton experimented with sunlight transmitted into his darkened chamber through a pinhole $\frac{1}{42}$ part of an inch in diameter. "For 21 of those Pins laid together took up the breadth of half an inch"; he measured the breadth of the fringes, three in number, at various distances from the hair, and extended the observations to include the fringes produced by a sharp knife edge and also by a narrow triangular slit formed by two knife edges inclined at a small angle to each other. Of these appearances he attempts no explanation, stating that at the time he was interrupted, "and cannot now (1718) think of taking these things into consideration. And since I have not finished this part of my Design, I shall conclude with proposing only some Queries in order to a further search to be made by others," and the first queries ask:

*Stearn]* *[Cambridge.*

FIG. 10.—Statue by Roubiliac in the ante-Chapel of Trinity College, Cambridge, given to the College by Dr. Robert Smith, Master, 1742–68.

"Do not Bodies act upon Light at a distance and by their action bend its Rays, and is not this action (*cæteris paribus*) strongest at the least distance? Do not Rays which differ in Refrangibility differ also in Flexibility? Are not Rays of Lightin passing by the edges and sides of Bodies bent several times backwards and forwards; with a motion like that of an Eel? And do not the three fringes of coloured Light above mentioned arise from three such bendings?"

In Query 25 we come to the double "refraction of Island Crystal described first by (1670) Erasmus Bartoline and afterwards more exactly by Hugenius in his book 'de la Lumiére' (1690)." The properties of the crystal and its effects on light are described, ending with the Query (26):

"Have not the Rays of Light several sides endued with several original Properties?"

This is discussed and proved, leading to a further Query (28):

"Are not all Hypotheses erroneous in which Light is supposed to consist in Pression or Motion propagated through a fluid Medium? . . . for Pression or Motion cannot be propagated in a Fluid in right Lines beyond an Obstacle which stops part of the Motion, but will bend and spread every way into the quiescent Medium, which lies beyond the obstacle?"

As a further objection he writes, after a reference to Huyghens' experiments on polarisation:

"For Pressions or Motions, propagated from a shining Body through an uniform Medium, must be on all sides alike; whereas by those Experiments it appears that the Properties in their Rays of Light have different different Sides."

Thus Newton is led to the conclusion expressed in Query 29:

"Are not the Rays of Light very small Bodies emitted from shining Substances? For such Bodies will pass through uniform Mediums in right Lines without bending into the Shadow which is the Nature of the Rays of Light."

In 1675 he had been less positive; in his series of

"Hypotheses as to the Nature of Light," submitted in that year to the Royal Society, he wrote, after premising the existence of an ether in which waves were set up by the impact of light rays on a reflecting or refracting surface:

"In the fourth place therefore I suppose light is neither æther nor its vibrating movement, but something of a different kind propagated from lucid bodies. To avoid dispute and make this hypothesis general let every man take his fancy. Fifthly, it is to be supposed that light and æther mutually act upon one another."

Newton was aware of Huyghens' "Traité de la Lumiére," with its beautiful construction for determining the position at any instant of a wave front, in a medium either crystalline or isotropic, as the envelope of the wavelets proceeding from all points of a previous position of the front; but such a construction did not account for rectilinear propagation. He was aware, also, of the fact that two waves in water of the same period reaching a given point by paths differing by an odd multiple of half a wave produced rest, and had thus explained the tidal phenomena of the port of Batcha in Cochin China, an explanation which one hundred years later first put Young on the track of interference; but he failed to see how this would account for many of the experimental results he had so clearly described.

Newton knew, also, that sound waves in air were propagated by disturbances taking place in the direction in which the waves are travelling, and, for him and his contemporaries, the waves in the luminiferous medium possessed this same property. Such a motion could not account for polarisation, and though he refers several times in the "Optics" to the vibrations of a stretched string, no one in his day had conceived that the vibrations to which light is due could, like those of the string, be transverse and give to the ray the sides needed to account for polarisation.

A century passed before Newton's doubts were resolved, and for that period his views held the field until, on Nov. 12, 1801, Young applied the principle of interference to explain the diffraction of light, the colours of thin plates, and many of the other phenomena discussed by Newton. Fresnel's first memoir on diffraction is dated 1815, and was followed in rapid succession by other researches by which he established securely the consequences of interference, and showed how it, when combined with the extreme smallness of the wave-length of light, led to an explanation of the rectilinear propagation. His memoir on the diffraction of light was crowned by the French Academy in 1818, exactly a century after the publication of the second edition of the "Optics."

Newton's second difficulty was solved at the same time. Hooke had suggested, but without any convincing reasons, that the vibrations in a light ray were transverse to the ray. Fresnel in 1816, in a Memoir on the influence of polarisation on the mutual action between two rays of light, makes the same hypothesis. Young in 1817 (article 'Chromatics,' Supplement to "Encyclopædia Britannica") writes:

"The elementary motions of the particles of the luminiferous medium are supposed to be principally confined to the line of direction of the undulation, while the most sensible effects of the waves depend immediately on their ascent and descent in a direction perpendicular to that of their progressive motion. . . . If we assume as a mathematical postulate in the undulatory theory that a transverse motion may be propagated in a straight line, we may devise from this assumption a tolerable illustration of the subdivision of polarised light by reflexion on an oblique plane."

But, writes Verdet, "The idea of transversal vibrations appeared a mechanical absurdity to all contemporary men of science," especially to Laplace and to Arago. The latter could never in his life decide to admit it as true, and Fresnel abandoned for a time an explanation based on this hypothesis.

In 1819, Arago and Fresnel published a Memoir on the mutual action between two rays of polarised light, and as a result of this, in 1821, Fresnel adopted the hypothesis of transversality in his Memoirs on mechanical considerations on the polarisation of light and on double refraction. One other experiment was required to disprove the emission theory, and this was first carried out by Foucault, who showed that light travelled more slowly in water than in air in accordance with the requirements of the undulatory theory. Newton's theory, as we have already seen, required the reverse result.

Another century has passed, and we have the quantum theory of radiation enunciated by Planck in his famous paper of 1901. Radiant energy leaves a radiant body, not continuously but in definite 'chunks'—quanta—depending on the frequency and on a constant, Planck's constant $h$, of which the value is $6{\cdot}55 \times 10^{-27}$ erg sec. The energy of the system is not equally distributed among the various modes of vibration, as required by classical mechanics; but little is found in short-

period waves; it is mostly concentrated in the low-frequency vibrations.

The law of radiation developed by Planck has been fully verified by numerous experiments, and the inverse problem, given that the final partition of energy is that determined by Planck's law, what laws of motion must be postulated for the system in order to obtain this law, was first solved by Poincaré (1912), who wrote: "L'hypothèse des quanta est la seule qui conduise à la loi de Planck."

Light is one form of radiant energy. Light then leaves a luminant body in quanta—light quanta—and this view has been developed by Einstein in his papers on light quanta (1905) and the theory of radiation (1917). Such a theory has its difficulties; it contradicts at first sight much that we know from experiment about the interference of light. Thus Prof. H. A. Lorentz (quoted by Jeans in his "Report [1] on Radiation and the Quantum Theory") said at the Birmingham meeting of the British Association in 1913: "Now it must, I think, be taken for granted that the quanta can have no individual and permanent existence in the ether, but they cannot be regarded as accumulations of energy in certain minute spaces flying about with the speed of light. This would be in contradiction with many well-known phenomena of interference and diffraction. It is clear that if a beam of light consisted of separate quanta, which of course ought to be considered as mutually independent and unconnected, the bright and dark fringes to which it gives rise could never be sharper than those that would be produced by a single quantum."

While it is tempting to try to draw an analogy between Newton's corpuscles and light quanta, and possibly between his fits of easy reflexion and transmission and the probabilities of periodic changes in the states of motion of an atomic system from which the emission of quanta arise, there are difficulties in the way of the acceptance of any mechanical theory purporting to account for the emission of light, which, up to the present, have proved insuperable, unless, indeed, we may see in the new mechanics developed by Heisenberg and Schrödinger some means of bringing the various experimental results under some general law.

In a recent paper read before the Royal Society on the quantum theory of the emission and absorption of radiation, Dirac studies the interaction of an assembly of light quanta with an ordinary atom, and claims to show that it gives Einstein's laws for the emission and absorption of radiation.

[1] For a discussion of the quantum theory, and of course of the problems here alluded to, reference should be made to this Report.

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Supplement to NATURE

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# Newtonian Time Essential to Astronomy.*

By Sir JOSEPH LARMOR, F.R.S.

*" Let us now praise famous men."*

## THE HISTORICAL PROBLEM.

WHEN Newton undertook the unprecedented task of exploring the dynamics of the motions of the heavenly bodies, sparsely scattered in the vast depths of cosmical space, the preliminary and novel problem in front of him was naturally to settle their bearings. He formally established the principle that, though there may be one essential space, or æther, all spatial frames of reference are equally permitted in material dynamics provided their motion relative to this standard frame and so to one another is one of uniform translation. Thereby, for example, he was justified in referring the internal motions of the planetary system to the sun as if it were a fixed centre, although it is in motion with velocity then unknown: going even deeper, he showed how the apparent regular revolution of the directions of the stars could be proved, by experiment, to include them within the range of a frame of the dynamics constructed locally so as to be appropriate to the events on the earth as a whole.[1] But he curtly laid down the postulate that there is one universal reckoning of time. He even went on, in another context in more speculative vein, to assign to this physical absolute time a transcendental significance,[2] which has made it a battle-ground of philosophers from Leibniz onward. On these fundamental topics no degree of precision can be excessive.

The next essential progress in general ideas relevant to permitted frames of reference for dynamical science came from James Thomson, the engineer brother of Lord Kelvin, as late as 1884.[3] He recognised that the totality of such permitted frames form an interchangeable group: he invented for them the name 'frames of inertia,' which has been recently supplanting the term Galileian co-ordinates introduced in the relativity theory. He even propounded the fundamental question: given three systems of bodies in independent motion in the same space, under what conditions is it possible to discover a moving frame of reference in that space (and therefore a group of frames) relative to which their motions are all uniform translatory? The very idea of a frame of reference, as something requiring a name, seems to have originated with his direct and acute, but sometimes tedious, thought. He recognised the problem of simultaneity, but could not solve it: that had to wait for the modern group-theory. About the same time (1870) C. Neumann published discussions, that attracted far wider attention, in which motions are framed by being made relative to 'the body Alpha.'

In the higher theoretical astronomy rotating frames of reference have however been practically in use ever since Newton, but more formally since the brilliant initiative of Euler in referring the dynamics of a rotating solid body to itself.

The simplicity of the Newtonian frames of space and absolute time was disturbed practically, by the fact that our celestial knowledge comes by rays of light which suffer delay in transmission. Principles of correction, taking cognisance also of the motion of the observer, were introduced by the great practical astronomer Bradley, during Newton's life-time, which have remained adequate until this day.

But close tests on the propagation of the light itself, initiated by Michelson (1881) after an idea broached by Maxwell, indicated that things pass as if the velocity of light were practically the same absolute constant within all local environments, unaffected by the directions and magnitudes of their convections through space. This result, then unexpected and even paradoxical, became the stimulus to modern projects of complete local relativity, based on a general postulate (1900) that each local scheme of events is, so far as relates to the atoms of matter, self-contained. Its theoretical proof, up to the second order of approximation even within the atoms if only their structure is electric, rested on a principle of correspondence which had in Lorentz's hands already (1892) given an easy explanation of the aberration of light and other first-order effects. Such theories, necessarily only approximate for application within the atoms, culminated in impressive practical form (1915) in the inclusion of gravitation by Einstein, leading to his astronomical predictions and their subsequent verification.

The confirmation (1919) of Einstein's prediction of the deviation of rays of light in the sun's gravitational field, at once placed these later developments of the dynamics of relativity in a privileged position, and made their critical study imperative. Later the eclipse result has been enhanced by a gradual consensus of expert opinion regarding the alleged gravitational influence on the atomic periods of free vibration: but the original close

* Communicated to NATURE early in February 1927.

[1] Cf. the weighty scholium at the end of the *Definitiones* in the "Principia," where Newton lays down the necessity of a formal foundation of *Axiomata sive Leges motus*.

[2] Cf. the condensed theological metaphysic of the final *Scholium Generale*; also the Queries at the end of the "Opticks."

[3] *Proc. R. S. Edin.*: "Collected Papers," pp. 379-403.

explanation, by the theory, of the outstanding small residue of the advance of planetary perihelia has become less cogent on account of uncertainties of the astronomical problem.

### Could Orbits be Isolated Geodesic Curves?

These predictions all rested on a postulate that in a planetary system of orbits, as represented symbolically in an Einstein-Minkowski fourfold continuum, each gravitational orbit is separately on its own account a quasi-geodesic curve, like a line of shortest length. But it has been held (1922) to be intrinsically impossible to reason on the basis that the warping of this auxiliary fourfold continuum is determined by the representative orbits within it, while at the same time each orbit is determined by the warping of the fourfold. The influences have to be treated as mutual: the necessary intrinsic invariance of Nature, holding amid all the accidents of arbitrary modes of reference, which at first pointed to the geodesic postulate as a suitable choice, must thus belong, as the invariant Hamiltonian Minimal Action does in the actual dynamical astronomy, to the system as a whole, not to the orbits separately. When, however, the orbital problem was examined [4] in the representative fourfold on this basis of a single mathematical Action function, originating in a field-Action, binding into a unity all the mutual influences, it appeared that the constant of gravitation had to enter into the expression for the Einsteinian absolute symbolic interval of fourfold space-time in the form $\gamma$ instead of $2\gamma$, leading to smaller potency of gravitation on light so that the two predicted optical effects of gravitation must each be halved.

The only criticism of this result of which I am aware was a remark to the effect that, if this is really so, then there must be something wrong with the application of the method of Minimal Action. Yet that method has continued to be, without substantial modification, the essential and eminently convenient foundation of the general fourfold mathematical theory of relativity. The astronomical verifications in agreement with the Einsteinian values, as they became generally accepted, have thus pointed insistently for several years to the necessity of finding some way of reconciliation of this alleged fundamental discrepancy in the theory. It was indeed clear enough that the rôle of astronomical time in the formulæ, as taken over from Einstein's own adaptation for his geodesic postulate, was unsatisfactory; but nothing better presented itself, though a slight error here involves great consequences. Various attempts at reconciliation, essayed by the writer during the last four years, have shown themselves inadmissible. It is easy, for example, to solve the problem, which is simply that of fitting gravitation within the domain of the optical and electrical relativity postulate, by means of a scalar potential of gravitation, introduced alongside the fourfold vector potential that is the expression of the electric and optical field: but that course, otherwise apparently unexceptionable, does not suggest any interaction between gravitation and light. Thus the actual astronomical detection of such an influence, as stimulated originally by Einstein's formulation, and carried through in the first instance in eclipse expeditions guided by Davidson and by Eddington with the help of the Astronomer Royal in the preparation, has now come to control the whole discussion in its own right.

[4] *Philosophical Magazine*, Jan. 1923, pp. 243-256.

### Dynamical Time Inherent in the Intrinsic Structure of Matter, not in Formal Frames.

The reconciliation of this discrepancy that will now be put forward is held to place the modern mathematical theory of relativity on its intelligible physical foundation. An actual astronomer is not an isolated solitary flying particle of the abstract mathematical theory. In his annual journeys at high speed round the extensive circumference of the earth's orbit, he takes his instruments and his landscape of reference along with him, without which he would be helpless: more important still, he takes his astronomical records extending over centuries of time, which have to be permanently intelligible and consistent to all brother astronomers, actual and potential, at all times and all places and moving in concert with their environments with all conceivable speeds. There must be something of the absolute involved here. The solution now offered is that our astronomer's own local space and time *are* absolute Newtonian space and time: that, unless we turn back from and reject the whole *corpus* of modern spectroscopic atomic doctrine, it is not possible to wriggle out of Newton's *dictum* of the necessity in dynamics of a universal continuous measure of time. Nor indeed do the creators of modern relativity attempt to do so, except in so far as many of them have become uncomfortable under the Einsteinian universal invariance of the time-space auxiliary fourfold interval, which as here interpreted involves and even imposes absolute time.

In the Einsteinian form of fourfold Action it is the modification of the time-like variable that dominates almost completely, as regards the actual universe with its slow motions, the forms of the symbolic tracks, because $t$ enters into $ds$ in the form $cdt$: when we have learned how to transform the symbolic track into an actual orbit in space and time with close accuracy as regards $t$, a much more rough adjustment will suffice spatially. Hence the importance of the utmost precision in the time-specification.

The extension of the environments of the astronomers, as enshrined in frames of the local standard, or so-called natural, space and time universal in character and the same for the environments of them all, into wider regions, is troubled by the fact that their only practical means of distant communication is by rays of light or other electric signals, which are delayed in transmission and affected by the motions of the observers. As regards astronomical observations that difficulty was discovered and surmounted

as early as 1727 by Bradley, who, especially by his fundamental principle of aberration, founded and even perfected the science of practical astronomy. But not merely an approximate practical consistency; an absolute mathematical fit is required, though possibly of a kind which may never attain to verification, if the accumulated work of astronomers is to be intelligible in ideal completeness to intellectual scrutiny, notwithstanding every variety of position and motion of possible observers and recorders and the complexities of light-transmission, of all which complications an ideal foundation for the records of astronomical history must in theory take exact cognisance.

It is held that the demand for universal intelligibility thus introduced, though of a generality which transcends any special *corpus* of physical science and so may appear at first sight to be unfruitful as being metaphysical, does point to a necessary and attainable actual construct within this domain of knowledge, that of astronomy and physics. Knowledge is rendered possible because, though the intrinsic times of all observers everywhere have to be identical because the local atoms are absolute in relation to their environments, yet the epochs or origins from which these times are measured in the various localities do not come into consideration at all, only intervals of time being concerned in the astronomical dynamics. It is this indetermination *à priori* of epochs, in conjunction with the absorption of all possible convections of local frames into simple invariance of the fourfold interval, that confers the requisite flexibility, permitting all the identical local reckonings of space and durations of time, of all observers everywhere in all ages, to be consolidated precisely within one wider auxiliary conglomerated fourfold scheme of mathematical representation.

The course of an astronomer's life, by the interpretation here advanced of the natural demand for relativity, works itself out of necessity under the sustaining local influences, is relative to his sub-permanent natural surroundings — this being indeed a type of postulate already recognised in mathematical form for similar reasons in the more minute cosmos of the atomic theory of elastic bodies. Our astronomer thus cannot know whether his environment is careering through space or not; that question must be meaningless to him, except so far as it is opened out by probable inference from the messages coming to his marvellously adapted sense of vision by light from the distant regions of the cosmos. In any case it is only his local smoothed-out frame of convenient reference, in which the occurrences that affect him are set, that can in any scheme of feasible simplicity thus be regarded as careering through space. He can and does construct his own Newtonian dynamical science of the motions observable within his local frame, often involving large relative velocities, even including local so-called centrifugal forces; and he could do this effectively though with far more trouble without calling upon light at all, just as if he and his associates were blind. Unless indeed the motion of the suitable local frame relative to adjacent permitted frames proves to be so erratic as to involve conspicuous differences of speed in its own adjacent parts, such as are called rotation: and then the remedy is to reduce the importance of the rotation, as can be done by increasing, by subdivision of frames, the fineness of the meshes of the analysis, so diminishing the extent of the local landscape which is the differential element, effectively uniform, with which a quasi-geometrical theory operates. Rotation of frames of reference, modifying velocities to an extent depending on the effective radius of the frame, appears on this view as a secondary and avoidable feature: in illustration, the rotation of the local element has added no term directly to the usual dynamical equations for a differential element of volume of a strained elastic solid.

The fact that the Lorentz transformation has to relate to convection without rotation is thus not now a harmful restriction. Moreover, on our view the influence of rotation can in any case be confined within the local frames, for it can be taken to belong to the dynamics of the bodies in the frame and not to the frame itself: a law of centrifugal force need not now belong to an unlimited frame and so extend away in increasing intensity, to be compensated at infinity. The original Newtonian argument from a spinning bucket, or in equivalent actuality the Foucault pendulum or gyro-compass, can demonstrate with the aid of light that the relative invariant property expressing fixity of gyrostatic directions within the frame, which the internal local Newtonian dynamics involves and can discover locally in self-contained manner, does actually extend outside as far as the fixed stars, which thus prove to be included within the region of spatial extension over which the play of physical theory is effectively Newtonian or uniform.

The directional fixations, unlike time, are thus local, not absolute; and the relevant question is, how far does the frame, for which they can be taken practically to subsist, extend: thus removing an insistent but, like unbounded centrifugal force, an unnecessary absolute. Or conversely, if we adopt as the expression of the natural notion of relativity, this idea that the convenient frames of reference of dynamics cannot be expected to extend uniformly to infinity or further than they can be tested, it points naturally towards trying a variable continuum, in which indirectly, as one may even say, Einstein discovered a suitable symbolic representation for an invariant gravitation. On the other hand, our immediate personal domain of local dynamics of matter can be so small that optical relativity merges practically in Newtonian relativity, the delays and other complications of light becoming negligible: but there is nothing to exclude another wider uniform frame, regarded as flitting across it, whose contents may be when so desired referred to it instead of their own frame, by aid of the Lorentz transformation which conserves the optical invariance all over it. This is the essential feature emphasised here, that the relations of pure relativity are concerned only with the specifications of localities in space and time,

and prescribe the modes of adjustment and continuation of the group of the permissible local frames of reference: that the mutual dynamics of the masses existing in each local frame proceeds independently by Newtonian principles, if, as in Nature is actually the case, they arise in a landscape practically permanent as regards the larger features each within its own most convenient frame.

For example, on this formulation of the natural notion of relativity, an astronomer will be under no temptation to undertake a violent journey through the cosmos in order "to return on the previous night" as has been said and so rejuvenate the activities of his life, any more than he will be tempted to go to the antipodes in order to stand on his head.

Only relations are directly accessible to our knowledge, and in this domain most conspicuously of all. No progress has yet been made, any more than in Newton's day, in unravelling the essential nature of gravitation. No reason can be assigned why it is just as intense as we find it, or why it exists at all. What can be established by the Einsteinian verified predictions is that, being in some unknown way an essential feature of the physical universe, it does fall into line, as one had anticipated that it ought to do, with electrodynamics and optics in obeying the principle of local relativity, the regional self-containedness of local material phenomena, as above elaborated.

### Dynamics in a Two-Dimensional Cosmos.

The analogy, and contrast, drawn from the Gaussian intrinsic geometry on a surface of variable curvature, by which these beautiful recent developments in a fourfold mathematical theory of universal relations in space and time were doubtless suggested, can be pushed a considerable way in this direction. We consider now not an individual explorer, but a whole population, whose activities are confined close to the surface on which they subsist, their thin cosmos of which alone they are conscious. Each community have their local world of events occurring in their local landscape on this sheet, and may construct, with a view to express the orderly succession of these events, their own local science of dynamics and physics. This science is conveniently laid down for each of the groups within a local frame of reference, which is flat or Euclidean, and can be envisaged as a uniform reference-lattice of straight lines; also in a local scale of time regarded abstractly as flowing uniformly, while determined practically by the prominent uniformities of recurrence in their phenomena. But when their region is extended too far, a misfit in their simple scheme rapidly develops; for the surface of their activities curves round, as we outsiders know but the inhabitants do not.

A theory of a wider cosmos cannot, however, arise at all unless the local populations possess some means of communication with one another. Let us consider a forest of threads stretched tight over their surface, and imagine that messages can be transmitted along them from each group to the others: these threads are so far the analogues of our suitable paths for transmission of rays of light. Without them, all these local frames of flat geometries and their related dynamics in natural time would subsist, but in isolation; without a very refined use of them there could be no kind of relation established between standard axes defining the expression of directions, in any two of the local regions, just as there is no overt relation between the epochs of the absolute times in our local regions also internally self-contained. But such inter-connexions can be worked up for them by constructing an auxiliary universal geodesic geometry (for example, spherical trigonometry) resting for its expression on the stretched threads, a conglomerate scheme which includes and also sums up all the local geometries. Though unbounded for them, it may even be restricted essentially, unknown to the inhabitants, to a finite, even possibly cyclic, domain as a complete frame for their cosmos, if the surface expressing our external, but to them abstract or symbolic and difficultly explorable, frame for the expression of their experiences closes up like an ellipsoid or solid ring: thereby relieving essentially the indetermination of the infinite. Their local plane geometries are here implied, that is, are postulated, to be all identical in type and scale—also their local experiences to be of the same type and therefore the time in which they are naturally set—that is, they are absolute in their dynamics.

This type of scheme involves, and is secured in its widest generality by, the analytic postulate, after Gauss and Riemann, that the squared interval of length on their surface, expressible as a quadratic function of the differentials of any two functions defining any lattice of co-ordinate curves determining position on the surface, is transferable all over it without change of value after the manner of a terrestrial surveying chain, is in fact mathematically invariant and absolute. Yet really all that our present science of differential geometry is usually capable of dealing with is gradual transfer, explored step by step on the basis of local differential equations. Here the rays of light are more potent than the threads or the geometry, for they can translate directly a finite interval of time, expressed in absolute atomic periods, across an almost unlimited range in the cosmos.

This is something beyond geometry, which imposes itself on the formal pseudo-geometric continuum determined by the postulated fourfold invariant interval. This latter, as already remarked, is able to create the fourfold scheme, because it can condense local frames, with all speeds of convection, within one element. How far this extraneous physical feature, the presence of light, tightens up the pseudo-geometric scheme we do not now stop to inquire; except to remark that it accentuates the absoluteness of time, as *sui generis*, already suggested and indeed provided for by the different sign of the relevant term in the expression for the quadratic interval itself. Nor will we consider the more recent problem set by Weyl and Eddington and followed up by Einstein, which really is how far a much wider type of frame for events, imposing

only local relation between vector displacements of points, can be constructed so as to involve in itself, be consistent locally, and beyond by continuation, with the different and more tangible kind of foundation provided by postulation of invariant intervals such as distance—for it would seem, as has been already remarked, to be too loosely hung together to be identical with a geometric frame even combined with an arbitrary electric field, though such frames may well subsist inside it.

### Other Elucidations and Contrasts.

There may be some instruction also in an analogy of our Newtonian Absolute Time $T$ with the Kelvin Available Energy, $A$, in thermodynamics. At each temperature of a material system under discussion, intervals of $A$ are measured as definite amounts of absolute energy; which is in fact the essence of the perpetual-motion postulate of Carnot, there the analogue of the present one of a local absolute time. But in passing to a different temperature of the material system the origin or epoch of measurement of $A$ has been lost, and it becomes necessary to include $A$ as a feature in a wider auxiliary theory, introducing a new and universal formal quantity, the entropy of Clausius, in addition to energy and of different essence. But alternatively we can usually do without any such general scheme by relying, as is familiar in special problems, on special isolated cycles of change resting directly on the Carnot-Clausius principle, our general knowledge of Nature supplying the relevant foundation—just as here we might perhaps reason directly on the relations between the local Newtonian frames instead of merging them in the one fourfold consolidation which their law of mutual correspondence permits.

We may also perhaps illustrate in another different manner. The pseudo-spatial construct of Einstein-Minkowski has given us an inkling as to how the cosmos of discrete events in history may be laid out, as it would be present to the sensorium (to adopt Newton's term) of a divinity who would have knowledge of all occurrences, but necessarily in an incoherent manner; for coherence is foreign to direct awareness of the totality of things, being the compensation permitted to imperfect knowledge, to some degree artificially and arbitrarily, by relating the succession of events in some kind of co-ordinated fourfold index, or frame, of reference. This gradual development of coherence in the range of our experience in space and time, which is scientific method, must run into some calculus of representation, in this generalised problem provided by the multiple algebra of tensors. The mathematical theory, brilliant as it is, soon indicates that this consolidated point of view, apparently losing all distinction between past and future, would transcend human intellectual ability to develop, except in its very rudiments; at any rate until mankind have learned to deal with a formal pseudo-space in four dimensions with the same intuitive familiarity that they now cope with ordinary space in three dimensions.

On the other hand, the actual science of astronomy is an affair not merely of going on "observing coincidences of point-events" in the heavens, but of accumulation of coherent records extending over centuries, permanent human documents in which all the new observations must find absolutely consistent places. There can be no science without memory and without records. For it to be humanly feasible to find out, for example, from minute discussion in the long-continued records, whether there really is a very small residue, unexplained by Newtonian theory, in the precession of the apse of the planet Mercury, must demand, if even only as a practical measure, some definite way of reckoning duration that is independent of the accidents of place and time and motion of observers; just as the instinct of Newton briefly postulated. And when in the setting out of a timeless cosmic history in a fourfold transcendental scheme, our special human knowledge, in space and time, acquired in marvellous manner from the advance of optical science, that an atom say of hydrogen must be taken to be intrinsically the same whether it be in a seething star or on the earth, so is absolute, obtrudes itself into this hitherto merely abstract historical spread, and demands, what is indeed a very small fraction of what it involves, an absolute universal (Newtonian) time for itself in its own material environment, determined by its own permanent vibrational properties, science must eagerly grasp at this revelation as in fact bringing the Promethean fire down from heaven, and rendering, as the philosophers say, human knowledge possible in this domain.

If the present course of argument is valid, the essential proof hitherto that this inference from the absoluteness of the atoms is in fact the right one, is just the confirmation in actuality of the Einstein predictions as to the gravitational influence on light, especially on the period of vibration, which, as one has been forced to hold for the last four years, would otherwise rest on a foundation largely accidental. Without that direct astronomical verification, insistence nowadays on absolute local times might well seem an anachronism.

If we could contemplate history like gods, looking equally before and after, generalised matter and energy might conceivably present themselves, as the elaborated mathematical representation in the fourfold implies, each as a tensor having as many as ten essential components: and they might be perhaps within limits identical and interchangeable. But within the frame of structure of the absolute standard thought that is valid and exchangeable amongst the human race, there have been at length recognised these wonderful absolute atoms existing as matter in their own rights without any sign of ageing or decay, being still in Maxwell's phrase "the foundation stones of the material universe"; and there has also been gradually acquired an idea of conserved energy which it has been useful to postulate as likewise universal and fundamental: and these two stand out as independent features of the foundations on which physical science builds; though related by the circumstance that the electric field belonging

to an atom is proved mathematically to add to its inertia, otherwise naturally an absolute possession, by virtue of the energy of that field.

When two atoms approach and, by overlap of their fields, release in the form of radiation some of these intrinsic field-energies then partially superposed, a proportionate part of the inertia of the atoms goes away along with it, subtracted from their translational energies when the atoms become separated again unless indeed they have suffered permanent internal change in the process. This works out consistently: energy of electric activity, even of free radiation, carries inertia with it, and thus carries momentum too. But the assertion that the whole of the mass of an ultimate atom is energy would remain a barren though permissible[5] figure of speech, unless it means to assert a postulate that mass is somehow practically all separable electric and other field-mass arising from *motional* energy of merely formal nuclei with separable æthereal fields. This would compete with the idea of an electron as a field of *static* strain-energy essentially and irrevocably locked together around a centre. A development by Minimal Action, as indicated in what follows, can provide room for both kinds of mass, the intrinsic and the exchangeable, within the postulate of absoluteness of the atoms.

### Gravitation remains Unexplained.

The original case for the postulate that orbits are separately geodesic curves in the gravitational auxiliary fourfold was based, very forcefully, on the Newtonian identity of mass and weight, which otherwise remains a challenge, as an unexplained universal feature of matter. According to the marvellously precise results of Eötvös, working with his modification of the Michell-Cavendish torsion-balance, the force of gravity is found to be the same per unit inertial mass as exerted on all kinds of matter, this being actually verified beyond the order of $10^{-7}$ which is nearly as far as the order of the observed optical and electric relativities, namely $10^{-8}$. If we reason on the basis of the Faraday type of concept, thinking however of a pre-ordained unchanging field of gravitational activity, then a collection of bodies composed of various kinds of matter move down the field in company, though with acceleration, not separating if they start with the same speed, almost as if they drifted in a current of fluid. If this field of gravitation is interpreted as merely a warp in a frame of space and time, this would naturally be regarded as a modification of the inertia which makes every free body describe, after the manner of Newton's First Law of Motion, on its own account alone, a representative path in a space-time auxiliary fourfold, determined by the purely quasi-geometrical property of minimal intrinsic quasi-length: and that is the gist of the geodesic postulate. But the other side of this consolidation of inertia with amenity to gravitation presents itself, when we reflect that a field of gravitation can vary, and so should have an assignable origin: which Newton found in an influence emanating from all the masses in the field, yet of a type of exceeding simplicity functioning just as if it were simple attraction according to his inverse-square law.

The Einstein mathematical theory can determine, at any rate as here amended, by self-consistent algebra in an abstract space-time fourfold extension, as a domain symbolically enshrining paths and masses of the bodies, a warp which proves to be necessarily restricted to a special type—just what is needed to represent gravitation. It has still to assume, however, after Newton and without explanation, that the mass which operates in this different function, that of creating the field, also is identical with the inertia mass. One may indeed reply that the geodesic postulate says nothing about mass: but it does determine a track along which the entity that determines the gravitational warp of the fourfold continuum is distributed, much as a magnetic field is determined by the tracks of electric currents,—a track which as above cannot be both direct cause and direct effect.

Though indeed the direct astronomical evidence to verify this identity of attracting mass with inertial mass may not as yet be very exact, the principle reposes firmly on a basis of its own, different from that of the Newton-Eötvös result; namely Newton's consideration that the accelerating influences between two masses rigidly connected together must be exactly balanced if motion is not to increase spontaneously without limit and so destroy any steady order of Nature. The argument from Eötvös carries only part of the way: this Newtonian doctrine of conservation of momentum is essential as well. But the latter is mutual dynamics of bodies, rather than geodesic geometry of an isolated track in a warped pseudo-space. It involves mutual relation between all the bodies in the field, bodies which also create the field: and this concurs with the previous conclusion that the path of each single body can hardly be regarded, except by an argument moving round in a circle, as a minimal inertial track in a continuum which that track itself has a share in modifying or even creating.

A study, in the light of this point of view, of the crisp exposition of principles in the first four sections of Einstein's earliest formal exposition of 1916 shows how easily it could have been turned round into the present direction. His 'natural' or absolute time, here adapted into the forefront of physical theory, is there practically put away in favour of a fourfold variable, transcending space and time,[6] and necessary to the auxiliary algebraic tensor theory, which is no more time than entropy is energy. It was natural in advance to presume that the symbolic path of a body, at any rate of an infinitesimal particle, was expressible as a minimal inertial track as if in a pre-determined continuum, until it appears as here maintained that this could not conform to actual gravitation as one mutual force between two bodies.

[5] But only up to the second order.

[6] It seems to have imposed itself originally in a very interesting exploring discussion, 'on the influence of gravitation on light,' *Ann. der Physik* (1911), § 3, English translation, p. 105, prior to the opening out of the general problem.

The discipline known as the mathematical theory of relativity would on the present view become a condensed census of the mutual necessary relations of the group of permitted frames of reference, in which investigators variously situated in space, time, and motion may conveniently and consistently formulate the physics of the local worlds, of infinite variety, to which they belong: but the cosmos is far more than its frame or even than our most far-reaching yet still superficial analysis of it within that frame.

It is a modified Newtonian physical foundation that has here been summarily set out. After all, if the present view justifies itself, it will be no derogation to the brilliant scheme of mathematical co-ordination of the general relations of gravitational and electric fields worked out by Einstein, and indeed not pressed by him (originally at any rate) to any transcendental issues. And on any view we have to admit that gravitation, like the great bulk of the rational exact principles of uniformity discovered by science in Nature, physical and biological, still in essence continues to lie beyond our scrutiny.

### Relation of Absolute Time to the Auxiliary Fourfold Hypercosmos.

It is becoming widely recognised—it seems now to be Einstein's own considered opinion—that an absolute transferable interval of space-time, the analogue of a universal measuring chain, cannot be avoided, however eager be the quest for an unconditional relativity of physical knowledge. It is here maintained that the difference in sign gives the time-element a footing independent from space in that symbolic absolute interval: in a sort of analogy to this, the general fourfold invariant tensor has been recognised recently as involving fundamentally, for the abstract relativity theory, two independent tensor *data*, a symmetric one and a skew one. When absolute time is thus taken out, the remaining part of the interval, giving the foundation for spatial determinations, is to be associated with an absolute lattice-frame locally uniform or Euclidean.

The pseudo-spatial auxiliary fourfold continuum of Einstein is made up by fitting together, so to say by dovetailing, the sub-groups consisting of all the local convected frames of reference (frames of inertia) which are suitable to enshrine the local Newtonian dynamics of ordinary experience belonging to the various environments in the cosmos. These variously convected local frames of reference, adapted for each domain of experience, but all condensed into one element in the auxiliary fourfold, which is effectively differential in the sense of differential geometry, may be presumed, in the search for a *locus* for gravitation, not to fit together into one universal group symbolically extending over the whole fourfold pseudo-space, without some straining of each of them, such as would cumulate sensible effects at places further removed from the centres of their localities. An essential feature is that this warping is here put on the frame, in preference to an equivalent warping of the laws of the local Newtonian dynamics that the frame enshrines. It is now well recognised that it might be put on either: the frame might be taken to be absolutely uniform throughout the universe, at the cost of complicating the expression of the experience formulated within it.

The essence of relativity as a practical proposition is that a scheme of knowledge is relative to some suitable frame of reference: it may be a matter of facility of exposition how much of universal relations is put into the frame, and how much is treated as belonging to the local dynamics of the system of bodies whose phenomena are referred to that frame. This is the key to the present treatment: everywhere there is the convenient frame of reference: it is relative to that basic frame, not to one another, that the local events are most simply and concisely expressed: such local frames, convenient and practically indispensable, have to be grouped together, consolidated into one compound scheme in order that scientific knowledge gained in the locality, in space and time, of one of them should be transferable into the localities of the others. That becomes possible, because all local frames, however differently convected, can be consolidated within one element of the fourfold by the Lorentz transformation, so that, the convections being thus disposed of, only space-time distributions remain to be fitted together, by strain if that proves to be necessary for the convenient expression of Nature.

The choice, however, of the local frames has here developed into more than a matter of convenience. The thorough absoluteness of the symbolic Einsteinian interval, involving, as here asserted, absolute time as well as a universal measuring chain for all local spaces, now working in concert with the marvellous practical instrument we have come to possess for exploring material systems far, outside our own dynamical environment, allows us not indeed to envisage directly the universal symbolic composite frame which subsumes all the actual local ones, but to carry through experimental tests for the formal validity of any attractive mathematical representation thereof that we may be able to contrive. The rays of light enable us actually to transfer directly the intervals of $t$, one of the fourfold auxiliary set of co-ordinates in this composite universal frame, being that one which is more specially related to time, from one part of the universe to another.

The differentials ($\delta t$) of this co-ordinate that are associated with the ray-pulsations stand locally in connexion with intervals of absolute time ($\delta T$) which are intrinsic for all environments, by a relation that may involve local gravitation among other things: it is the spectrometer that can provide the test whether this constructive process for communication with distant systems is in actuality verified,—and that in a way that Newton could hardly even have imagined. First the absolute period of vibration ($\Delta T$) of the solar hydrogen atom is to be transferred into an equivalent interval $\kappa\Delta t$ ($=\Delta T$) in the universal auxiliary co-ordinate $t$ of the minimised fourfold; then the very same absolute period, namely $\Delta T$, of a terrestrial hydrogen atom may be observed,

and also can be transferred into the equivalent interval $\kappa'\Delta t'$ expressed in this co-ordinate $t$ by the ratio $\kappa'$ appropriate to terrestrial instead of solar environment; thus giving $\kappa\Delta t = \kappa'\Delta t'$. As it is the auxiliary period $\Delta t$ that is transmitted by undulation along the ray and received into the spectrometer, that instrument functions *as if* it were measuring directly two different actual periods $\Delta T/\kappa$ and $\Delta T/\kappa'$. For vibrating atoms at rest in the solar frame $\kappa = (1 - 2V_s/c^2)^{\frac{1}{2}}$ where $V_s$ is the gravitational potential of the sun: for a terrestrial atom $\kappa'$ is practically unity. This agrees with the Einstein prolongation, now only *apparent* not intrinsic, of the periods of the solar radiation. If the atom were moving with speed $v$ in the solar frame of reference, instead of being at rest, the Lorentz transformation as *infra* would seem to impose another factor $\epsilon^{-\frac{1}{2}}$ or $1 - \frac{1}{2}v^2/c^2$, on the period, which is in the opposite direction but wholly swamped by the Doppler effect.

This feature, that every interval $\Delta t$ is conveyed without change all along the representative ray-path of the auxiliary fourfold, is based on the nature of the undulation along a ray; it rests in theory on the circumstance that the electrodynamic optical equations, as transferred into the invariant form inherent in this composite fourfold ($xyzt$), are satisfied in undulatory manner, not exactly but to adequate approximation, by making the variable $t$ enter in the expression of the solutions only through a universal periodic factor $e^{ipt}$. It is the absoluteness of the hydrogen or other atom, steadily maintained through all vicissitudes of place and motion even doubtless of acceleration of its environment, referred to its own frame that accompanies it, for which internally the translatory motion means nothing, that provides a universal natural measure of an absolute flux of time $T$, namely that of any one of its intrinsic types of vibration: it is the spectrometer alone that in quite recent times has become available to extend our local dynamical experience practically and directly into a universal scheme of intervals of time everywhere absolute, such as is involved symbolically in the universality of the Einstein space-time measuring interval.

In terms of universal Newtonian time thus acquired, the permanent results of astronomy have to be expressed: for its accumulated records must be in accord with the local measuring appliances, pendulums, gyrostats, marks of local direction, position, etc., on which observers wherever they be in space and time and however convected have to rely. This postulate of local frames of reference in space and time, enshrining their material dynamical contents, everywhere Newtonian and absolutely identical just because dead matter and its manifestations are presumed to be identical in type everywhere, is already latent in the preconception of a *continuous* differential geometry applicable to the fourfold: that the frames can be thus consolidated therein, conveniently and manageably to sufficient approximation, along mathematical methods such as the one that Einstein has opened out, is from the present point of view consistent with our fundamental requirement that as knowledge proves to be possible and enduring, the modes of its acquisition and record by all potential observers of natural phenomena, however variously situated in the cosmos, must be consistent among themselves.

### Theoretical Coherence is ensured under Least Action.

It remains to indicate briefly how these ideas modify essentially the mathematical analysis and its interpretation, without unduly disturbing Einstein's famous three tests, postponing for the present other issues that incidentally arise.

We base the whole development, after Einstein, Hilbert, and Lorentz, and recently Whittaker who has attempted a striking electrodynamic generalisation, with ideal security and simplicity, on one invariant scalar integral, extended over the fourfold conglomerated representative continuum in which the symbolic history, past and future, of the cosmos is spread out, namely,

$$A = \int (G + L + \lambda) d\tau.$$

That integral is the closest available analogue, in this mixed ($xyzt$) continuum, of the Action integral of Lagrange and Green framed in ordinary space with independent time, as applied to develop the laws of undulatory disturbance whether in a continuous elastic material body or in an æthereal optical medium. Here $L$ is the analogue of the Action-density of the elastic or electric field; $\lambda$ is a constant which may be needed to satisfy the restrictions of our point of view, the analogue of a pressure which was familiar in early optical theory as necessary to adjust across an interface undulations purely transverse like those of light, occurring in a medium supposed to be incompressible; $G$ is a multiple of the Riemann scalar curvature-invariant which confers just the suitable intrinsic freedom when the basic fourfold is now contemplated after Einstein as non-uniform, while its element of extension $d\tau$ then involves the familiar scalar factor $\sqrt{g}$.

This formulation of a compound Action-density, which is to become expressive of universal dynamical history, is first to be minimised as regards its distribution, with reference to variation of structure of the fourfold pseudo-space, as in the end controlled by its material contents expressed symbolically by their historic tracks therein. This variation leads to structural differential equations of that fourfold, thus conferring on it a definite character of the Riemannian geometric type. Within the pseudo-spatial connecting structure thus established, the Action can now be integrated by parts, leading to boundary terms along with a different spatial integral: it may be that the spatial integral vanishes, and this will occur under quadratic conditions. The boundary for such integration in the fourfold consists, in an atomic theory, of the surfaces of very thin filaments enclosing the historic tracks of the atoms and electrons which alone are out of bounds: the boundary integral is reducible practically to line-

integrations along those tracks. The Action $A$ may thus, on account of the structure of the field as already settled by its own variation, be expressible as made up of parts associated with the material system alone as represented by the historic tracks of the atoms or finite masses. It is now to be further minimised [7] with respect to variation of these historic tracks, that is, of the representation of the history of matter: that will lead to the expression, suitable to the fourfold, of the dynamical equations of interaction of the atoms, in fact to a *symbolic* dynamics of the matter that is present in the field, now latent, which itself arises from that matter as nuclei or mathematical singularities of suitable type, in accordance with the laws determined by the previous variation in the field itself: all this mutual complexity being held firmly in check by its origin from a single Action formula.

THE GUIDANCE OF CLASSICAL ELECTRIC THEORY.

This process is directly suggested, and has even been guided, by cognate Maxwellian electric theory: [8] the special case of a system of linear electric currents in space affords a close analogue of the historic linear tracks of atoms. In that theory the energy of the currents—there a kinetic part $T$ and a potential part $W$—is postulated to reside, possibly as strain and motion, in the dynamical field of the interconnecting medium or æther. An Action-density $L$ in this medium, or kinetic potential $-L$, is formulated analytically, being equal to $T-W$. The Action $\int L d\tau' dt$, $d\tau'$ being spatial, thus expressed by local elements, is minimised over all the field, thereby adjusting the electrodynamic field to a coherent structure, presented in the form of its Maxwellian absolute circuital equations. With this field-structure, so determined, the energy, and the Action, become condensible into expressions in terms of the currents alone that are the sources of the field, in the form of line-integrals involving their circuits: the result as regards the kinetic part is represented by F. Neumann's energy formula, on Amperean lines, for linear currents in terms of the ancient current elements such as $\iota ds$, namely a mutual energy

$$T = \Sigma\int\int \frac{1}{r_{pq}} \iota_p ds_p \iota_q ds_q \cos(ds_p ds_q).$$

Incidentally and more precisely, when the current is expressed in ultimate form as made up of electrons, the complete Action, as it has now to be, in form suitable to the fourfold, is

$$A = \Sigma\int\int \frac{1}{r_{pq}} (e_p dx_p e_q dx_q + e_p dy_p e_q dy_q + e_p dz_p e_q dz_q - c^2 e_p e_q dt_p dt_q);$$

for this form is invariant (except in one feature as regards $r_{pq}$ which leads to a different story that would now carry us too far) and involves the two sets of independent variables $(x_p y_p z_p t_p)$ and $(x_q y_q z_q t_q)$ in the fourfold, expressing position of each pair of interacting electrons but not any one universal time $t$. Reverting to Maxwellian theory, the forces between the bodies carrying the electric currents are then determined by further variation applied to the Neumann formula, now with respect to change of form of their circuits, in time, by variation with respect to position without further reference to the field, still in the background but already previously settled: and that in fact constitutes the Neumann energy-theory. If we wish to avoid specifications in terms of any concept of mutual forces, this process of minimising a scalar Action by variation can lead direct to the complete set of equations of motion of the conductors that carry the currents.

One essential point the analogy brings out prominently. It will not do, after Hilbert and most writers on this subject, merely to add some suitable invariant form of line-integral to the field Action $A$ in order to express the interactions of the material atoms. The Faraday-Maxwell doctrine is that the energy, or in complete general dynamics the invariant Action, all resides in the field, there being no other; coherently, in that the field adjusts itself minimally to laws that permit this energy, other than free energy expressed as radiation, to be associated permanently with its sources, the atoms or electrons constituting the currents, and thus to be treated as belonging to them.

We have to determine what form the corresponding generalised Action, thus reduced to material form, ought to take in the fourfold problem. The Neumann formula suggests a double integral extended along each pair of the circuits or historic tracks. Under certain limitations (now postponed) it is reducible to a single integral, analogous to the Maxwellian kinetic form involving the electric potential $(FGHV)$, namely

$$\Sigma\int e_p(F_p dx_p + G_p dy_p + H_p dz_p - c^2 V_p dt),$$

involving with Maxwell $dt$ not $dt_p$. There arises also naturally here, in regard to the fourfold, a local part deriving from mutual activities of the nature of self-induction between the sub-filaments of the cylinder-track of the moving electron, which adds a term $\mu_p ds_p$, where $\mu_p$ is a constant expressive of the familiar electric inertia; this part is directly suggestive of the modified Einsteinian intrinsic inertial form now to be introduced, to which an electric part like the above is to be added as the reduced expression of electrodynamics.

PREVIOUS ANALYSIS BY ACTION REVEALED DISCREPANCY WITH FACT.

In the previous effort towards this type of gravitational theory, already referred to (*Phil. Mag.*, Jan. 1923), a linear form of integral was assumed for this reduced Action, as being the nearest analogue of the Einstein geodesic form which connects itself so readily with dynamical orbits. This restricted the choice to the only available type of invariant linear form, which is

$$A = \Sigma\int -cm_p ds_p;$$

where locally in each frame $ds^2$ reduces by suitable change of co-ordinates to the absolute intrinsic standard form $c^2dT^2 - d\sigma^2$ where $d\sigma^2$ is Euclidean, while all over the fourfold it is expressed con-

[7] This is not the procedure for determining *actual* orbits in space and time. This reduced Action has the requisite invariance, from its mode of formation: and it can now be transferred from the timeless fourfold into a frame in space and time as *infra*, when it becomes the ordinary dynamical Action of the planetary system as referred in space and time to that frame.

[8] Cf. "Æther and Matter" (1900), Ch. vi, especially §§ 56-9.

tinuously in terms of universal co-ordinates by an invariant quadratic differential.

We now introduce the Schwarzschild expression for the spatial-gravitational field as modified by the symbolic track of a mass $m$ moving with changes of its speed restricted to be very slow compared with the velocity of light, namely

$$ds^2 = c^2\left(1 - \frac{km}{c^2 r}\right)dt^2 - \left(1 - \frac{km}{c^2 r}\right)^{-1} dr^2 - r^2 d\theta^2 - r^2 \sin^2\theta d\phi^2.$$

Or better for the problem of several interacting planets, the earlier but less exact Einstein expression

$$ds^2 = c^2\left(1 - \frac{km}{c^2 r}\right)dt^2 - \left(1 + \frac{km}{c^2 r}\right)(dx^2 + dy^2 + dz^2),$$

which is spatially isotropic and so adapted to superposition of disturbing influences. Our direct immediate concern is the question whether, in order to express gravitation within the scheme if that proves to be possible, $k$ must be identified with its absolute constant $\gamma$, or with $2\gamma$, the first alternative halving the gravitational effects on light as originally predicted. With sufficient accuracy for this purpose we have for the field of a system of planets the collective isotropic form

$$ds_p = cdt_p\left\{1 - \Sigma\frac{km_q}{c^2 r_{pq}} - \left(1 + \Sigma\frac{km_q}{c^2 r_{pq}}\right)\frac{v_p^2}{c^2}\right\}^{\frac{1}{2}},$$

$$v^2 = \frac{dx^2}{dt^2} + \frac{dy^2}{dt^2} + \frac{dz^2}{dt^2},$$

from which by further approximation in which the square root is expanded, keeping only the most important terms, we obtain for the Action the expression

$$A = \int \Sigma dt_p\left(\tfrac{1}{2}m_p v_p^2 + m_p \Sigma \tfrac{1}{2}\frac{km_q}{r_{pq}} + \ldots\right) + \text{constant}.$$

Now compare this with the ordinary dynamical Action from which by itself alone the entire system of equations of motion of any planetary astronomical system is derivable, namely,

$$A' = \int (T - W)dt = \int \Sigma dt(\tfrac{1}{2}m_p v_p^2 + \tfrac{1}{2}m_p V_p), \quad V_p = \Sigma\frac{\gamma m_q}{r_{qp}},$$

where $V_p$ is the gravitational potential of the other masses at $m_p$. The former expression involves a plurality of independent co-ordinates $t_p$ as free variables, one belonging to each body on its historic track: the latter has the one time-variable $t$. The former variables must somehow be reduced to a single one, in order to be expressive of the progress of history from stage to stage.[9] In default of anything better, we previously, following the usual tacitly adopted course, simply substituted the same $dt$ for each of these independent differentials $dt_p$; as the result we had

$$A = \int \Sigma dt\left(\tfrac{1}{2}m_p v_p^2 + \tfrac{1}{2}\frac{k}{\gamma}m_p V_p\right).$$

On that pure assumption it proves to be possible to establish the necessary agreement between $A$ and $A'$, merely by equating $k$ to $\gamma$: whereas the geodesic postulate that each orbit is determined by itself alone from its own equation $\delta\int ds_p = 0$, with the same identification of $dt_p$ with $dt$, would obviously, with Einstein, make $k$ equal to $2\gamma$.

Thus by use of the single invariant mutual Action, instead of the plurality of geodesic forms which as we hold are only spuriously invariant, both optical effects of gravitation were, on adopting perforce the usual procedure, reduced to half their observed values.

The weak link in the argument was recognised at the time, and has been already here sharply indicated. As nothing better suggested itself, the usual course was followed by replacing the independent co-ordinates $t_p$, $t_q$, . . . expressive in part of the positions of the bodies on their tracks in the fourfold, by one universal time-variable $t$. In another aspect, instead of conducting a general variation of position in the fourfold, it was restricted to displacements confined to hypersurfaces transverse to $t$; which is not an invariant or intrinsic process, because $t$ may belong to any slicing whatever of the fourfold.

### Newtonian Time now introduced.

We now claim to be in a position to do better, as the Newtonian absolute time $T$ has been acquired. The $t_p$ is the variable of the type of time that is appropriate to the material system $m_p$ referred to the convenient frame of our problem in which all the local systems are taken as moving, this one with velocity $v_p$. It can be placed in relation to $T$, which is the time-variable appropriate to its own frame, changing relative to the others, in which $m_p$ is always at rest, by a Lorentz transformation expressed at each stage, if $x_p$ is taken for brevity along $v_p$, by equations of type ("Æther and Matter" (1900), p. 174: Lorentz, *Proc. Amsterdam Acad.* (1904): Einstein, *Ann. der Physik* (1905))

$$\delta X = \epsilon^{\frac{1}{2}}(\delta x - v\delta t), \quad \epsilon = \left(1 - \frac{v^2}{c^2}\right)^{-1},$$

$$\delta T = \epsilon^{\frac{1}{2}}\left(\delta t - \frac{v}{c^2}\delta x\right).$$

Here, now introducing subscripts, $\delta X_p$, $\delta T$ belong to the particular planet's own frame, $\delta x_p$, $\delta t_p$ belong for each planet to the solar frame to which the motions of all the planets have to be referred in a theory which includes them all. It is the transfer from $\delta t_p$ to the universal $\delta T$, which is none other than the (reversed) familiar very slight relativity shrinkage of time, of order $10^{-8}$, that rises to be an essential feature for orbits because $\delta t$ always occurs with a factor $c$. The transfer is effected by the relation $\delta t_p = \epsilon_p^{-\frac{1}{2}}\delta T$; provided we can ignore the addition $-v_p/c^2 . \delta x_p$ to $\delta t_p$ in the formula, which expresses a change of epoch of $T$ with locality analogous in Lorentz's striking comparison to change in civil terrestrial time for different longitudes. If our local standard frame of inertia, that is, such that dynamics of its material content is practically uniform and Newtonian, is not of too great extent, this omission makes no practical difference: the theory has however to remain approximate, though abundantly covering actuality, in this as in other respects, leaving the practical issues for systems involving speeds of a higher order than planetary still a riddle.

We can now introduce into the Action this unique absolute time $T$ as subsisting intrinsically within every moving system $m_p$. But it may still be objected that $t$, as usually introduced in place of

[9] It is just the negation of this consideration that has led to deposing time into an accident of place and motion, and so to abolishing history.

$t_p$, $t_q$ . . . could be regarded as the absolute time on the sun, to which the motions of all the planets are thereby referred. This remark may suggest a contradiction in our processes: but it is removed by reflecting that though $T$ is measured in the same unit on the sun and on a planet, being absolute, yet it is measured from different epochs, which we cannot specify, and which do not keep constant for the succession of elements of $T$ because the planet's own frame of $T$ is continually changing relative to that of the sun. The only $\delta T$ that is a continuous differential of a function $T$, for the environment of the planet in which the process of variation is carried through, is by Newtonian postulation that one which constantly belongs to it, with its optically vibrating atoms, when referred to a frame continuously carried along as part of itself. It is this $T$ that is the unambiguous cumulating time of the records of the astronomers, who work each in his own essential local landscape in which, as in all others, it inheres.

Reverting after these explanations to our analysis, by a simple substitution for $dt^2$ within the radical, we now have immediately, using isotropic forms for $ds^2$, with sufficient approximation [10]

$$A=\Sigma\int -c^2m_p\left\{\left(1-\Sigma\frac{km_q}{c^2r_{pq}}\right)\left(1-\frac{v_p^2}{c^2}\right)dT^2-\left(1+\Sigma\frac{km_q}{c^2r_{pq}}\right)\frac{d\sigma_p^2}{c^2}\right\}^{\frac{1}{2}},$$

$$=\Sigma\int -c^2m_pdT\left(1-\Sigma\frac{km_q}{c^2r_q}-\frac{2}{c^2}v_p^2+\ldots\right)^{\frac{1}{2}},$$

$$\text{as } dt_p=\left(1-\frac{v_p^2}{c^2}\right)^{\frac{1}{2}}dT,\ d\sigma_p^2=dx_p^2+dy_p^2+dz_p^2=v_p^2dT^2,$$

the product terms of higher order under the radical in $A$ now, be it noted, cancelling out. Comparison with the Newtonian Action now restores $k$ to the value $2\gamma$ as with Einstein; but the result may still differ (see, however, footnote *infra*) by second order terms from the Newtonian Action in a way which might affect sensibly the secular advances of the perihelia of the planets.

We claim in fact to have discovered how to transfer, with sufficient approximation, the reduced form of Action, constructed in the conglomerated fourfold in order to secure the requisite invariance, into the solar frame in space and time of the planets, which is competent to contain it and will lead to the development of dynamical astronomy by further variation in the usual manner with respect to the planetary positions.

Introduction of the factor transforming from the co-ordinate $dt_p$ to a universal time $dT$ has thus affected the kinetic energy of each body as it occurs in the reduced integrand of the Action with a factor 2, together with minor changes: and to restore large-scale agreement with the astronomical Action $A'$ the potential energy has to be affected with the same factor, which requires us to make $k$ equal to $2\gamma$ instead of $\gamma$. This new inconvenient common factor 2 would then be absorbed by amending the original Action form to $A=\Sigma\int-\frac{1}{2}cm_pds_p$, a change that will presently prove to be essential as regards the relation of mass to energy.

[10] If a track $p$ is regarded as made up of filaments which influence one another the summation $\Sigma km_q/c^2r_{pq}$ ought to be increased by a constant $k_p$ which is a sort of gravitational self-induction like that of an electric current on itself. This would function as a field-addition to mass, did it not cancel out as occurring in two places with opposite signs: there is no field-inertia of gravitation.

But a point arises here that demands close scrutiny, as it may be a main source of obscurity. Reduced to standard local form the transferable invariant $ds$ is expressed by $ds^2=c^2dt^2-d\sigma^2$; as $ds$ is thus $cdt$ for a frame in which the $d\sigma^2$ of a track locally vanishes, it has been customary in the theory to call $c^{-1}ds$ standard time. If this is meant to be the same $dT$ as the present interval of absolute time, it involves the relation

$$dT=c^{-1}ds=\left(1-\frac{v^2}{c^2}\right)^{\frac{1}{2}}dt=\epsilon^{-\frac{1}{2}}dt,$$

whereas our present result, derived from an immediate comparison of frames, is the direct opposite one, $dt=\epsilon^{-\frac{1}{2}}dT$. The justification for it is, briefly, that the Minkowskian fourfold determined by invariance of $ds$ is not a frame of reference for matter in motion; its element of extension is rather a complex of local frames with all speeds of convection. Neither matter nor motion is discernible within the fourfold: it is on the present view a necessary consolidation of the relations of the permitted (locally inertial) frames, in any one of which the actual world can be framed as bodies in motion. But it bears traces of this composite origin, from groups of local frames with all possible motions of convection, in the historic tracks which are indirectly symbolic of the movement of bodies as exhibited in any of the merged constituent frames.

The $c^{-1}ds$, though it has been called standard time, has nothing to do with any moving material system: it is to the local material system itself in its own frame, as the essential *datum*, not to any or all of the other extraneous permitted frames of reference in their various convections relative to it, that the absolute time belongs. Though perhaps a relation between the epochs of $T$ in two local systems is determinable ideally by counting the alternations in $t$ along the undulatory ray connecting them, yet as time progresses the ray rapidly changes owing to their motions and no practicable relation could ensue. Or, expressed differently, the frame of the planet is constantly changing relative to the solar frame, which prevents a steady difference of time-epochs between their frames.

When we come, however, to determine the deflection of a ray passing near the sun it is the spatial gravitational field adjacent to the sun with which we are concerned. Referring the rays to the frame of the sun, the electrodynamic theory shows that the coefficient of $dt^2$ in the expression for $ds^2$ expresses with adequate approximation the square of velocity of light near the sun, while the curvature of the ray is by Huygens' principle the local gradient of velocity along its normal.[11] Thus the Einstein deduction for the deviation of the ray holds good with his coefficient $2\gamma$ as now restored, but on a different foundation, in place of $\gamma$.

### Relation of Electric Mass to Energy demands Newtonian Time.

The point has not yet been considered that the time-like co-ordinate $t$ in the fourfold is not unique; for it is one of the impressive features in general relativity-theory that any mode of slicing of the

[11] This needs closer elucidation, now postponed.

hypercosmic fourfold may equally well express it. But what we are concerned with in our actual approximations is the simplified case which Levi-Civita has called Einsteinian statics, in which $ds^2$ involves no products of $dt$ with other differentials: the ambitions of the wider abstract theory are scarcely practicable, and may be excessive. The motions of all local systems which have speeds small compared with light are then represented by historic tracks, now in a real $(x, y, z, ct)$ fourfold, which are almost parallel: and the co-ordinates $t_p$, $t_q$, ... of the various systems are suitably measured along some direction for $t$ nearly parallel to them all. It is this simplification which makes an approximate development of the fourfold scheme practicable for actual astronomy, while leaving its relations to actuality unsolved for higher speeds.

It will be noted that in circumstances where the gravitational potential is negligible, the direct inertial part of the Action, as now expressed by

$$A = \ldots + \Sigma \int -\tfrac{1}{2} c m_p ds_p,$$

where $$ds_p = c dt_p \left(1 - \frac{v_p^2}{c^2}\right)^{\frac{1}{2}}, \quad dt_p = \left(1 - \frac{v_p^2}{c^2}\right)^{\frac{1}{2}} dT,$$

becomes $$A = \ldots + \Sigma \int -\tfrac{1}{2} c^2 m_p \left(1 - \frac{v_p^2}{c^2}\right)^{\frac{1}{2}} dt_p$$

$$= \ldots + \int \{\text{const.} + \Sigma \tfrac{1}{2} m_p v_p^2\} dT,$$

when each system is referred to its own intrinsic absolute time; thus indicating that essential mass of an atom, as distinct from inertia derived from an attached field to some degree separable, is an intrinsic constant $m_p$ unaffected by its relation to frames of reference, as naturally it ought to be.[12] It would only be electric field-mass that is affected by its velocity in the frame, being related to field-energy relative to the frame.

Moreover the latter relation, fundamental though it be, is *not* substantiated at all except on the present Newtonian scheme. For an electric system it is common ground that the relevant reduced Action, of fourfold invariant type, must be of form, again with the new adjusting factor $\frac{1}{2}$,

$$\tfrac{1}{2} \Sigma e_p \int (F_p dx_p + G_p dy_p + H_p dz_p - c^2 V_p dt_p),$$

where $$F_p = \Sigma e_q \dot{x}_q / r_{qp}, \quad V_p = \Sigma e_q / r_{qp}.$$

For a static electric system it is thus, closely,

$$-\tfrac{1}{2} \int \Sigma c^2 e_p V_p dt_p, \text{ becoming } -\int W dt,$$

where $W$ is its electrostatic energy, when all its electrons are travelling together so that $t_p$ is the same for all. On introducing Newtonian time, but not otherwise, this is

$$-\int W \left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}} dT,$$

which is the familiar form of Action indicative of varying inertia according to a law which makes the energy $W(1 - v^2/c^2)^{-\frac{1}{2}}$, thus showing its dependence on the translatory motion. For example, to our second order of approximation this Action is

$$\int \left(-W + \frac{W}{\tfrac{1}{2} c^2} v^2 + \ldots\right) dT,$$

indicating an increase of inertia of amount $W/c^2$.

[12] In a field of large uniform gravitational potential $V_p$ an increase of inertia of $m_p$ depending on $V_p/c^2$ is simulated: but that point of view is confused. The Action is the sole arbiter, which here determines the total result of the field as orbital motions.

## Conclusion.

The conservation of Newtonian absolute time for local material systems, as supplied by their intrinsic vibrating atoms of matter, within the Einstein mathematical method of fitting the existence of gravitation into the optical relativity, has thus led to his value of the displacement of spectral lines, also as we have seen to his value for the ray-deflection, but not to his result for the precessions of planetary perihelia.[13]

It is noticeable how little use of the mathematical tensor theory is required for the general argument on the scope of relativity: it here provides only the form of $ds^2$ for the gravitational field within the solar system, as affected by each mass that is present, and that form is required only to a rough approximation, in order to identify a representation of gravitation within the formulation by $ds^2$ of the invariant fourfold, though closer calculation is necessary in order to determine the deflection of actual rays and the precessions of planetary perihelia.

Finally it will be observed that the present scheme has to be throughout an approximate one, leading as is claimed in a natural manner to the two optical effects of gravitation as now widely accepted. It works with frames of ordinary space and time, correlated in an auxiliary mathematical fourfold. It may be regarded as the continuation of the previous approximate scheme of electric equivalence of frames, of long ago, "Æther and Matter" (1900), Chapters vi, xi, which at that date covered adequately all the verified facts of optical and electrodynamic relativity, and justified the postulate of its universal validity at any rate up to the second order: without noticing, however, as Lorentz afterwards discovered by an equivalent independent electrodynamic formulation, that the formulæ on which it was based are obviously valid without approximation for the Maxwellian field; though valid only problematically, if at all, within the domains of the atoms, except to the second order of approximation there regarded as imposed on that account. The hypothesis of unrestricted exactness, regardless of structure in the atoms, has eventuated in wide and profound formulations towards hypercosmic schemes, which constitute the modern mathematical theory of relativity. The physical interpretations here advanced rest of course on the most brilliant Einsteinian fourfold device for involving gravitation within optical local relativity; that seems to have now become justified,—but as here urged when it is reconstructed into more consistent and Newtonian setting—by the astronomical tests, as the right type of mathematical formulation; in contrast with modes, one for example making use of a scalar gravitational potential in the fourfold, that might otherwise be in competition with it.

[13] When the approximation to the reduced Action for the Sun-Mercury system is carried to the second order, it appears that its form comes out of the type of the Newtonian Action for an elliptic orbit. If this is confirmed, then on the present theory there would be no influence on the secular progress of the perihelion: which, amid the astronomical complications, and in face of the smallness of the outstanding residue, can scarcely now be regarded as an unsatisfactory conclusion. The masses have to be predominantly intrinsic, as in the text.

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## Tyndall's Experiments on Magne-crystallic Action.[1]


By Sir WILLIAM BRAGG, K.B.E., F.R.S.


IN 1845, Faraday made the surprising discovery that the vast majority of substances, not merely iron, nickel, and cobalt, are affected by a magnet: and showed also that the action is repulsive quite as often as attractive. Faraday's results excited the greatest interest and were the starting-point for many other researches. In fact, they paved the way for the work of Thomson and Maxwell, who came thereby to the establishment of the laws of electromagnetism. Among the many workers who followed Faraday was Tyndall, who made certain interesting discoveries relating to the behaviour of crystals in the magnetic field.

A very lively discussion sprang up as to the mode of interpretation of the new discoveries, particularly that of the so-called diamagnetism. On one hand, Faraday was satisfied that he could describe them in terms of his 'lines of force': the majority, including Tyndall, referred everything to the existence of poles, magnetic and diamagnetic. Tyndall's experimental work, and the consequences which he drew from it, were devoted to the support of these views. When Faraday's conceptions prevailed it became clear that Tyndall's interpretation of his own experiments must have been incorrect. His collected account of his researches, published in the well-known "Diamagnetism and Magne-crystallic Action," never became a link in the chain of argument.

The recent analysis of crystal structure by means of X-rays throws some new light on those experiments of seventy years ago. We can see more clearly where Tyndall's conclusions were in error. But at the same time the experiments of Tyndall are seen to be closely related to a modern research of immense importance, that of the effect of stress on the constitution and physical properties of materials.

[1] Discourse delivered at the Royal Institution on Friday, Jan. 21.

### FARADAY'S FIRST OBSERVATION OF DIAMAGNETISM.

On Sept. 13, 1845, Faraday made one of his most important discoveries, that of a relation between magnetism and light. He found that when plane polarised light was made to traverse a piece of his 'heavy glass,' a borosilicate of lead, in a direction coinciding with that of lines of magnetic force, the plane of polarisation was rotated. He had thus

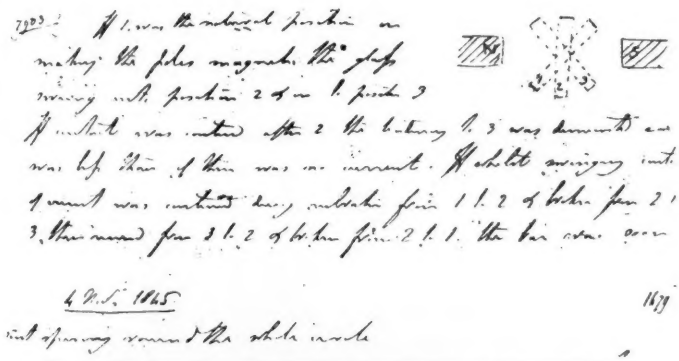

FIG. 1.—The above figure and a few lines of explanation are photographed from Faraday's original notes.

been successful in showing that the action of a magnet did not require the co-operation of a magnetic substance such as iron for its manifestation, but might be directly connected with a substance of a different kind, namely, glass, and a different activity, namely, light. In the following months he tried to find some other connexion between magnetism and this glass. He floated his glass on a liquid and tried whether he could move it by a magnet, without result. But on Nov. 4 he succeeded in his search.

"The bar of heavy glass, $1\frac{6}{8}$ of an inch long and . . .[2] of an inch square, was suspended by cocoon silk in a glass jar in principle as before and placed between the poles of the last magnet. When it was arranged and had come to rest I found I *could* affect it by the magnetic forces and give it position. Thus touching diamagnetics by magnetic curves

[2] Not filled in, but from other notes we know it was half an inch square.

and observing a property quite independent of light by which we may probably trace these forces into opaque and other bodies as the metals, etc."

"If 1 was the natural position on making the poles magnetic the glass swung into position 2 and on to position 3. If contact was united after 2 the tendency to 3 was diminished, *i.e.* was less than if there was no current. If whilst swinging contact of current was continued during vibration from 1 to 2 and broken from 2 to 3, then united from 3 to 2 and broken from 2 to 1 the bar was soon sent spinning round the whole circuit" (see Fig. 1).

The word 'diamagnetic' is here used to denote substances through which, on his views, magnetic lines were passing. It is not yet used as an anti-

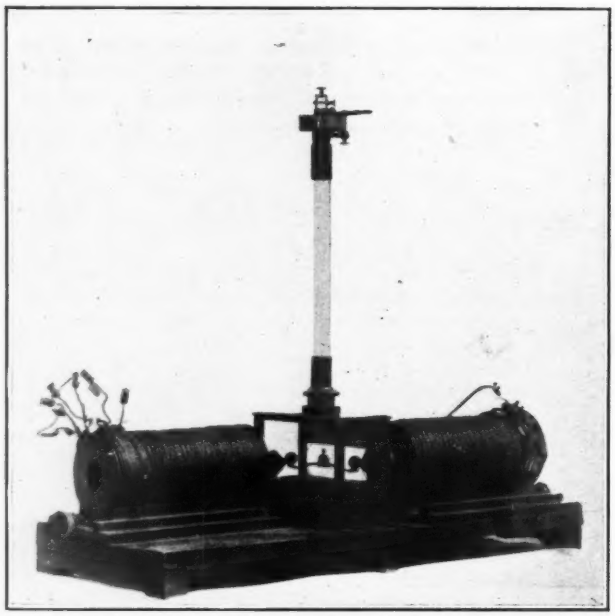

FIG. 2.—Apparatus used by Tyndall. Two balls of bismuth are placed at opposite ends of an arm swinging at the end of a suspension thread. The electromagnets are staggered so as to combine their actions in turning the arms by means of the repulsive forces exerted on the balls. ("Diamagnetism and Magne-crystallic Action," p. 53.)

thesis to 'paramagnetic.' His new result obviously gave him intense pleasure, and in following it up he was so preoccupied that he did not even go to the meeting of the Royal Society on Nov. 20 when his paper on the "Action of Magnets on Light" was read.

We can easily repeat the experiment, using a piece of the same glass taken from the store left by Faraday. It is not the actual piece, Number 174, as he tells us in his notes, for this cannot be found. The glass turns slowly in the magnetic field, and its motions are obviously controlled by switching the current off and on. The glass tends to set itself *across* the lines of force running from pole to pole, not *along* the lines as a piece of iron would do; and obviously the effect is very small as compared with the violent movements of iron in the same circumstances.

The action may be described as a repulsion of the glass by the magnet: and sometimes the early workers on the subject constructed apparatus specially designed to show the repulsive effect more obviously, and to distinguish it from a mere turning action in a magnetic field, if indeed this could be done. We can illustrate this point, and at the same time the diamagnetism of bismuth, by using a piece of apparatus constructed for Tyndall (Fig. 2).

### FARADAY'S FIRST EXPLANATION OF DIAMAGNETISM.

Faraday at first suggested that the diamagnetic effect was the antithesis of the ordinary magnetic effect. A piece of iron when placed between two poles became so magnetised that a south pole was developed upon it in that part which was nearest to the north pole of the inducing magnet and vice versa. Faraday's suggestion that the diamagnetic substance developed north and south poles where a magnetic substance would have developed south and north respectively was taken to be a satisfactory explanation. It was the constant endeavour of later experimenters to express their results in accordance with Faraday's hypothesis: even when development had reached a stage some distance ahead of that described in the original paper (*Phil. Trans.*, 1846, p. 21).

Faraday was himself the first to feel doubts as to the satisfactory nature of his explanation. His early results could conveniently be described as showing an exact antithesis between two classes; where one was attracted by a magnet, the other was repelled: where one set itself in a certain direction in the magnetic field, the other avoided that direction as much as possible. It seemed proper to describe them as being in exact antithesis to each other, and the word diamagnetism was adopted as a means of expressing the experimental result.

He prepared a list of substances which showed varying degrees of response to the action of the magnetic field, and the plan of the statement illustrates his first views ("Experimental Researches," Series xxi. No. 2424):—

| | |
|---|---|
| "Iron | Alcohol |
| Nickel | Gold |
| Cobalt | Water |
| Manganese | Mercury |
| Palladium | Flint glass |
| Crown-glass | Tin |
| Platinum | Heavy glass |
| Osmium | Antimony |
| 0° Air and vacuun | Phosphorus |
| Arsenic | Bismuth" |
| Æther | |

It is to be observed that those preceding air and vacuum are to be considered above zero or magnetic, those succeeding, below zero or diamagnetic, which is meant to imply a true antithesis.

In Dec. 1845 ("Experimental Researches," Series xxi. No. 2429), Faraday writes:

"Theoretically, an explanation of the movements of the diamagnetic bodies, and all the dynamic phænomena consequent upon the actions of magnets on them, might be offered in the supposition that magnetic induction caused in them a contrary state to that which it produced in magnetic matter; *i.e.* that if a particle of each kind of matter were placed in the magnetic field both would become magnetic, and each would have its axis parallel to the resultant of magnetic force passing through it; but the particle of magnetic matter would have its north and south poles opposite, or facing towards the contrary poles of the inducing magnet, whereas with the diamagnetic particles the reverse would be the case; and hence would result approximation in the one substance, recession in the other."

Even at that time, however, Faraday's views were not firmly established: and we may repeat an experiment of his which shows the nature of the contrary influences that were impressing him. A small glass tube filled with a weak solution of the magnetic substance iron sulphate sets itself axially between the magnetic poles; but if it is surrounded as it swings by a strong solution of the same substance, it sets equatorially. The tube appears to be magnetic as compared to air, but diamagnetic as compared to the strong solution.

Might not, on this analogy, all substances, and also air and vacuum, be magnetic, reacting to the magnet in the same way but to different degrees? And might not bismuth exhibit its peculiarities, not because it is in antithesis to iron, but merely because it is less magnetic than the air? Yet he writes as follows:

"Such a view also would make mere space magnetic, and precisely to the same degree as air and gases. Now though it may very well be, that space, air and gases, have the same general relation to magnetic force, it seems to me a great additional assumption to suppose that they are all absolutely magnetic, and in the midst of a series of bodies, rather than to suppose that they are in a normal or zero state. For the present, therefore, I incline to the former view, and consequently to the opinion that diamagnetics have a specific action antithetically distinct from ordinary magnetic action, and have thus presented us with a magnetic property new to our knowledge" ("Experimental Researches," Series xxi. No. 2440, Dec. 1845).

The extract describes his first-formed opinion.

### Plücker's Discovery of Magne-crystallic Action.

The next important step is due to Plücker:

"In 1847, Plücker had a magnet constructed of the same size and power as that described by Faraday, his object being to investigate the influence of the fibrous constitution of plants upon their magnetic deportment; while conducting these experiments he was induced to try whether crystalline structure exercised an influence" (Tyndall, "Diamagnetism and Magne-crystallic Action," p. 2).

The first experiment made by Plücker gave an immediate and decided reply. The investigation of the behaviour of several crystals led him to announce the following laws:

"When any crystal whatever with an optic axis is brought between the poles of a magnet, the axis is repelled by each of the poles; and if the crystal possesses two axes, each of these is repelled with the same force by the two poles.

"The force which causes this repulsion is independent of the magnetism or diamagnetism of the mass of the crystal; it decreases with the distance more slowly than the magnetic influence exerted by the poles."

There is some truth in Plücker's conclusions, but much correction is necessary. Tyndall pointed out in 1850 that they broke down completely when applied to calcium and iron carbonate. These two crystals are isomorphous; the former, Iceland spar, obeys Plücker's laws in that it sets its axis equatorially in the magnetic field, but iron carbonate sets its axis from pole to pole. Plücker had, however, done great service in directing attention to the peculiar behaviour of crystals in the magnetic field.

In the autum of 1848 Plücker was in London. Faraday writes in his laboratory notes:

"16 Aug. 1848. Plücker has described to me certain of his results as to the crystalline diamagnetic relation and, as I understand it, the optic axis of a crystal having *one* optic axis tends to pass into the equatorial direction, or if a crystal have two optic axes then the line between them tends to pass into the equatorial direction."

"25 Aug. 1848. To-day Plücker showed me for the first time some of his experiments.

"First Optical Results.

"A small rhomboid of Cal$^{e}$ Spar was suspended by a single cocoon thread between my Elect. Magnet poles with the optic axis in a horizontal position. When the poles were very close as in the figure the diamagnetic force of the substance made it take the position shown in which the optic axis is axial to the magnet. But when the poles were opened out to distance of half or threequarters of an inch, then the mass pointed axially and the optic axis therefore equatorially [see Fig. 3]. . . . There is a given distance between the Mag poles (pretty close) when a certain or piece
rhomboid ∧ of Cal$^{e}$ spar between them is so affected that the diamagnetic and the magneto-optic force is balanced, at smaller distances the piece points diamagnetic and at larger distances Magneto optic. So that on increasing the distance the magneto-optic force diminishes *less rapidly* than the magnetic force, and on diminishing the distance it increases less rapidly than the magnetic force. *But increasing or diminishing the strength of the magnet produces no alteration of this place of neutral action, it only increases or diminishes the strength of the action on each side of it: or rather the resultant of the two actions on each side of that neutral position.* So Plücker at least tells me, for I did not see that proved."

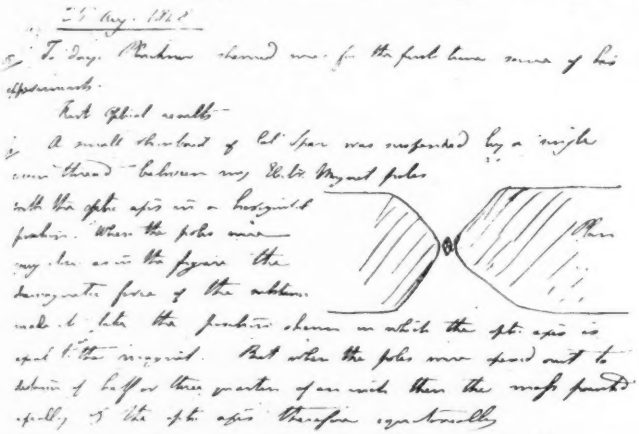

Fig. 3.—The figure is taken from Faraday's notes, Aug. 25, 1848.

Plücker's experiment is readily shown; but a little care in adjustment is required. The dimensions of an equal-sided rhomb are rather too much the same in all directions: a somewhat more irregular piece is, I find, easier to work with. The effect is much more clearly seen with a good bismuth crystal, which was obtained in the following way. A little bismuth was melted in a glass tube which had been drawn to a point. The tube was placed in an electric furnace, from which it was made by clockwork to emerge very slowly. The fine end of the tube came out first, and the bismuth at the point was the first to solidify into crystalline form. The rest of the metal crystallised slowly as the emergence proceeded, and, in the circumstances, continued the structure and orientation of the first fragment. In this way, due to Bridgeman, the mass contained large single crystals, not a mass of fine crystals as is usual when the solidification takes place rapidly.

When the crystal, which is ten times as long as it is broad, is placed in the magnetic field due to pointed poles, it sets strongly equatorially in accordance with the usual behaviour of a diamagnetic body: but when the poles are withdrawn somewhat, it sets axially with equal strength.

The experiments of Plücker introduced a new effect which Faraday afterwards called "magne-crystallic action." It clearly deserved a name, since its manifestations added a complication to the diamagnetism which had already been observed.

The new discoveries presented so many forms when repeated with different crystals suspended in different ways and with different forms of magnetic field that the complications were not unravelled for some years. Some of the difficulties were due to the circumstances of the experiments and had no relation to the real question. One of these incidental effects was that of attractions and repulsions due to transient currents induced in bodies already suspended for observation between the magnetic poles when the magnets were excited. As is well known, the motion of a spinning block of copper is at once arrested by the action of such currents; on the other hand, a sheet of copper held near a pole is sharply repelled when the current is turned off, and if properly suspended can be set into a rapid spinning. These effects had nothing to do with diamagnetism, but they were apparently the cause of confusion on some occasions.

Another great source of difficulty was the overpowering effect of iron impurities; the diamagnetic effects are so feeble in all cases that a mere trace of iron, nickel, or cobalt is sufficient to mask them. So, for example, Plücker's experiments with antimony seem on this account to have been at variance with the true facts as proved by Faraday (Tyndall, p. 16).

Faraday's Researches on Magne-crystallic Action.

In 1848, Faraday published a series of researches on the magne-crystallic phenomena, which cleared

up some of the difficulties. But in 1850 he could still write as follows :

"Four years ago I suggested that all the phenomena presented by diamagnetic bodies, when subjected to the forces in the magnetic field, might be accounted for by assuming that they then possessed a polarity, the same in kind as, but the reverse in direction of, that acquired by iron, nickel, and ordinary magnetic bodies under the same circumstances. This view was received so favourably by Plücker, Reich, and others, and above all by W. Weber, that I had great hopes it would be confirmed ; and though certain experiments of my own did not increase that hope, still my desire and expectation were in that direction. (2641) Whether bismuth, copper, phosphorus, etc., when in the magnetic field are polar or not is, however, an exceedingly important question ; and very essential and great differences in the mode of action of these bodies under the one view or the other, must be conceived to exist. I found that in every endeavour to proceed by induction of experiment from that which is known in this department of science to the unknown, so much uncertainty, hesitation, and discomfort arose from the unsettled state of my mind on this point that I determined if possible to arrive at some experimental proof either one way or the other. This was the more important because of the conclusion in the affirmative which Weber had come to in his very philosophical paper. . . . (2642) It appeared to me that many of the results which had been supposed to indicate a polar condition were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of action ; others, again, appeared to have their origin in induced currents. . . ."

Accordingly, he undertook a further series of researches which in the end brought him to regard all his effects as expressible in the simple form with which we are familiar. In his "Experimental Researches" he writes (Ser. xxvi., Oct. 1850, No. 2807) :

"When a paramagnetic conductor, as for instance a sphere of oxygen, is introduced into such a magnetic field considered previously as free from matter, it will cause a concentration of the lines of force on and through it so that the space occupied by it transmits more magnetic power than before. If, on the other hand, a sphere of diamagnetic matter be placed in a similar field it will cause a divergence or opening out of the lines in the equatorial direction, and less magnetic power will be transmitted through the space it occupies than if it were away" (see Fig. 4).

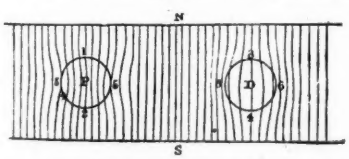


FIG. 4.—The figure is taken from Faraday's "Experimental Researches," and was drawn to show his conception of the passage of lines of magnetic force through paramagnetic and diamagnetic bodies respectively.

This describes diamagnetism generally : the complication of magne-crystallic action is described with equal simplicity :

"(2837) If the idea of conduction be applied to these magne-crystallic bodies it would seem to satisfy all that requires explanation in their special results. A magne-crystallic substance would then be one which in the crystallised state could conduct onwards, or permit the exertion of the magnetic force with more facility in one direction than another : and that direction would be the magne-crystallic axis. Hence, when in the magnetic field, the magne-crystallic axis would be urged into a position coincident with the magnetic axis by a force correspondent to that difference, just as if two bodies were taken, when the one with the greater conducting power displaces that which is weaker."

It is only a uniaxial crystal, of course, which possesses a single magne-crystallic axis ; it is the axis of a certain spheroid. The facility of conduction in different directions in a biaxial crystal requires an ellipsoid for its representation.

This way of stating the rules allows us to see at once the principle of the experiments shown by Plücker to Faraday, which the latter so greatly extended. When the magnet poles were close, the crystal occupied a part of the field where the lines of force were very divergent. In such circumstances the orientation of the crystal would be determined by the general tendency for diamagnetic bodies to move from the stronger to the weaker parts of the field, and the crystal set its longer dimension perpendicular to the field ; the optic axis was then parallel to the lines of force. But when the magnetic poles were separated the crystal covered a part of the field in which there was little divergence : the magne-crystallic action then took charge, and the crystal set itself so that the direction of worst conduction of the lines, *i.e.* the optic axis, was at right angles to the lines.

### Experimental Illustration of Magne-crystallic Action.

A few simple experiments will serve as further illustration of these rules. We take a crystal of sulphate of iron which has the form of a thin plate : the flat sides are cleavage planes and the 'conducting power' for Faraday's lines is far greater across the plate than along the large faces. In a uniform field the crystal plate sets equatorially therefore, and even when allowed to move up to one of the poles keeps its face normal to the lines. A thin plate of iron would stand on edge on the pole : but the magne-crystallic action of this paramagnetic crystal is exceedingly strong.

A bismuth crystal so suspended that its axis (it is a uniaxial crystal) is vertical has no magne-crystallic action. Its motions are governed by the general tendency of its mass to move from the stronger to the weaker parts of the field: in a uniform field it has no appreciable tendency to set itself in any particular direction. But when the crystal is hung so that the axis is horizontal, that axis tends strongly to set itself along the lines of force, as we saw before.

Naphthalene is a monoclinic crystal. Its magne-crystallic properties are represented by an ellipsoid, one axis of which coincides with the single axis of symmetry. The cleavage is very perfect and the crystals take the form of thin plates parallel to the cleavage planes. The axis is also parallel to the cleavage planes, and if the crystal is suspended so that this axis is horizontal and the cleavage planes vertical, the axis and the planes move into the equatorial position, in this way fulfilling symmetrical considerations. But when the crystal is hung in a uniform field, so that the axis is vertical, the cleavage planes set themselves at a certain angle to the field. One of the axes of the magnetic ellipsoid then lies along the lines, another across them: the third is vertical. There is no obvious relation between the cleavage plane and the first two axes. Finke (*Annalen der Physik*, **31**, 149; 1910) has shown that this may be said of various crystals examined by him. If now the crystal be suspended from the other end of the axis, its cleavage planes will make the same angle with the field but on the opposite side of the medial line (Fig. 5). Faraday describes results of this kind which he obtained with a paramagnetic crystal of sulphate of iron ("Experimental Researches," Series xxi. Nos. 2634-7). Naphthalene is diamagnetic: like many other organic crystals, it shows the magne-crystallic effect very strongly.

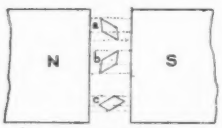


FIG. 5.—In this figure the positions marked *a* and *b* are positions of equilibrium of the naphthalene crystal in the magnetic field. The axis of symmetry is vertical and perpendicular to the plane of the paper. The cleavage plane is also vertical and its intersection with the plane of the paper is the longer side of the rhomboid. The form of the crystal shown in the figure is not a natural form, because the shorter side of the rhomboid is drawn parallel to one face of the cell of the crystal lattice, which face does not usually occur on the crystal. It is so drawn in order to show the relation between magnetic lines and the lattice.

If the crystal is hung from one end of the *b* axis, the position *a* is assumed, and if from the other, the position *b*. The position *c* is impossible.

These experiments will serve to show the great variety of effects that may be observed. All of them are, however, easily correlated by Faraday's conception of lines of force. Let us remember that there are several variables and give due importance to each. The first of these we call diamagnetism, implying that the lines pass through the substance in question less easily than through the air or a vacuum. The second is called magne-crystallic action, in reference to the fact that in a crystal the lines pass more easily in one direction than another. A third variable is the crystal shape, which may also affect the set in the magnetic field when the latter is divergent. A fourth is the amount of divergence of the field which, in a uniform field, falls to zero. After experimental disturbances have been allowed for, all these influences have to be taken into account.

The more divergent the field the more does the simple diamagnetic effect assert itself, and any magne-crystallic action which would tend to make the specimen set a crystal axis or axes at some particular inclination to the direction of the field is overpowered.

### The Contrast between Paramagnetism and Diamagnetism.

On the other hand, magne-crystallic action usually takes charge in a truly uniform field. For the sake of brevity and an easier explanation, it may be well to direct attention to a fact which was not fully appreciated by all the first experimenters, but was clearly set out by Sir William Thomson (Lord Kelvin) in 1885. A diamagnetic bar, apart from magne-crystallic action, tends to set itself *along* the lines of force in a *uniform* field, just as a paramagnetic bar. For we may imagine the bar to be made up gradually of a collection of cubes, placed successively one after the other in the magnetic field. The effect of the first cube is, as we should say in the language of Faraday, to spread out the lines of force on their way through the cube, and to crowd them together on either side of it. A second diamagnetic cube will, if free to move, go to that part of the field where the lines are least crowded; it will therefore avoid setting itself beside the first cube and prefer to place itself in front or behind it. A third will continue the same process, and in the end the cubes will form a bar pointing along the lines. In the case of a substance such as iron, there is a double converse.

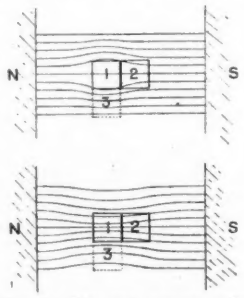


FIG. 6.—Dia- and paramagnetic substances in a magnetic field. In both cases Position 2 is preferred to Position 3.

The lines are most dense just in front and just behind the first cube; and a second cube will place itself in one of those positions because a magnetic substance seeks the strongest part of the field. Again, therefore, the bar grows along the lines, as in this case we know from experience (see Fig. 6).

It is quite certain that no one has ever seen the first of these effects, because it must be so minute and difficult to separate from others. We may safely infer it, as Thomson pointed out, because our theories of the electromagnetic field have been abundantly justified by other means. The susceptibility of bismuth, by far the most diamagnetic substance, is only about $10^{-6}$; in other words, the strength of the field on one side of a bismuth cube of 1 cm. side would only be about a thousandth part of 1 per cent. greater than at the front or back of the cube. Near a pointed pole the strength of the field might easily vary by 50 per cent. in a centimetre. It is easy to see how feeble is the force tending to arrange the supposed cubes parallel to the lines of a uniform field as compared with the forces acting on a bar placed near the pole.

The analogous effect in electrostatics can, however, be realised. When two plates are immersed in oil and maintained at a large difference of potential, an elongated rod of glass hung from a fibre sets itself along the lines of force (to make sure that the effect is true the rod must be free from any conductivity due to its own substance or a water film). This corresponds to the setting of a magnetic body in a uniform field. When bubbles of air are allowed to rise through the oil they are [3] drawn out along the lines of force; which effect, since the inductivity of air is less than that of oil, represents the setting of a diamagnetic body along lines of magnetic force.

When a diamagnetic substance sets itself across the lines of a magnetic field, and no magne-crystallic action is at work, it is because the field is not really uniform. It is perhaps a little confusing when it is said, as is sometimes the case, that diamagnetic and paramagnetic substances are the antitheses of one another in that one kind sets itself across the field and the other along it. This is only true of a field which is non-uniform. Indeed, it may be said that the use of the word antithesis is incorrect in any case. There would be a true antithesis if one substance could be defined by its pointing along the lines of force in one direction while another pointed in exactly the opposite direction; there is no true antithesis between pointing along the lines and pointing across them. It seems to me, though I say it with diffidence, that this difficulty was stirring in Faraday's mind and was the true cause of the uneasiness of which he spoke in a quotation given above, and of his aversion to the description of diamagnetism and paramagnetism as being the antitheses of one another.

Faraday, as I have said, when this thorough examination of the facts had led him to frame a hypothesis which would link them together, based his interpretation on the existence of lines of force, and found himself able to place both his own results and those of others in their place within a self-contained system.

Kelvin placed this hypothesis in mathematical form, thus completing the treatment of the subject of magnetism by Poisson; the latter had left out of his consideration the consequence of magnetic susceptibility being different in different directions, not because he overlooked the possibility of such an effect, but because no case of its occurrence was known to him.

### The Hypothesis of Polarity.

Faraday's views were not accepted, however, by other experimenters on the same subject, and in particular by Tyndall. The idea of polarity was not to be given up easily, and innumerable experiments were made to show that a 'diamagnet' had poles like a magnet, but in the opposite sense. A bar of bismuth would develop north and south poles when, in similar circumstances, a bar of iron would develop south and north. Of course, when the facts are prepared for mathematical treatment, they can be expressed in this way. It is generally convenient and justifiable to represent a magnet by two poles because the form of the field at any reasonable distance is satisfactorily represented thereby, though in the immediate neighbourhood the lines of a real magnet do not run like those of the theoretical bipole. Within the body of the magnet the lines run from the south pole to the north, continuing and completing their course outside so that every line is a closed circuit; but all lines near a bipole run from the north pole to the south pole. So also in the magnetic shell, which is in the mathematical treatment the exact analogue of the electrical condenser, the bulk of the lines run from one plate to the other across the narrow space between the two plates; comparatively few run from the outside of one plate, through surrounding space, to the back of the other. In the condenser, the internal field is the most important, the external being looked on as a correction. In the magnetic shell the reverse is the case; the outside

[3] My authority is Capt. Dunsheath of the Henley Telegraph Works Co.

field is that which is considered because it represents more and more closely, as the plates are brought closer together, the field due to a current circulating about the contour of the condenser. The theoretical charges on the plates have to be made larger and larger as the plates are brought together, so that the strength of the outside field may remain the same.

Now, if a piece of bismuth is placed along the lines of a magnetic field, the lines avoid the piece to some extent, though, as already explained, the avoidance is extremely small. If we take the bismuth away and replace it by a feeble bipole consisting of south-pole magnetism, of proper amount, where the lines come out of the bismuth, and a corresponding amount of north-pole magnetism where they go in, the whole arrangement being made in a vacuum, or permissibly, in the air, we get (Fig. 7) in this artificial way an external field resembling that which exists when the bismuth is in place. It can be said that polarity is developed in the bismuth in a sense opposite to that which is found in iron in the same circumstances. If the effect is to be represented, for the convenience of treatment, as due to the presence of a bipole, then the sense of the bipole in the case of bismuth is opposite to the sense in the case of iron. The old argument, therefore, was not between two hypotheses but between two languages in terms of which the facts were to be described. Such an antagonism, once believed in and debated, could be and was the cause of an immense variety of experiments devised to justify one side or the other. But Faraday felt that his way of putting the facts was more fruitful in suggestion of further experiments, and more convenient as a foundation for theoretical development. He has been abundantly justified.

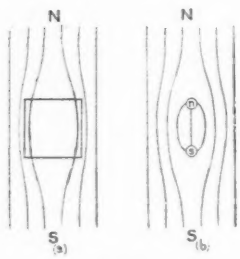

FIG. 7.

### The Work of Tyndall and Knoblauch.

We now come to the part which Tyndall played in a debate which was conducted on both sides in such an able and, it is pleasant to observe, in such a friendly way. In the first place, Tyndall and Knoblauch published in 1850 an account of experiments which they had made. They showed that Plücker's first views, those which included the repulsion of the optic axis of a uniaxial crystal by the poles of the magnet, were, as already stated, incorrect in many cases, and they substituted an amended set of rules in the following terms :

"If the arrangement of the component molecules of any crystal be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that chosen by the respective forces for the exhibition of their energy. If the mass be magnetic this line will set axial; if diamagnetic, equatorial."

The key-word is "proximity." This condensed statement of course requires explanation. Tyndall supplies it in full in his book on "Diamagnetism and Magne-crystallic Action." A very brief summary will be sufficient for our present purpose. In the first place, the observations made by him and his partner on the behaviour of crystals in the magnetic field convinced them that the plane of cleavage determined in a number of cases the position which the suspended crystal would take. Magnesium sulphate, zinc sulphate, saltpetre, and topaz were diamagnetic substances, and their cleavage planes, the crystals being so suspended that these planes were vertical, always set themselves equatorially, *i.e.* at right angles to the field. On the other hand, nickel sulphate, scapolite, and beryl, which were magnetic crystals, in the same circumstances set their cleavage planes parallel to the field.

The connexion between these results and the statement quoted above lies in this, that the molecules in a crystal were supposed to be in greater proximity along a cleavage plane than in any other direction. Let us take bismuth as an example; it is diamagnetic and sets its cleavage plane equatorially in accordance with Tyndall's rule. Its structure has now been determined by X-ray analysis, so that we can see what meaning can be attached to the claim for proximity in the plane of cleavage. The bismuth structure can be looked on, approximately, as a slightly distorted cube; one of the cube's diagonals has been a little stretched, while the other three have been left unchanged. The crystal is therefore uniaxial; the axis is the stretched diagonal. The principal cleavage plane is perpendicular to the axis. The spacing of the planes parallel to the cleavage is larger than that of any other set of planes in the crystal, and these consequently contain more molecules to the unit area than any other planes. Tyndall would have said that in those planes there was a maximum proximity between the molecules.

The X-ray analysis of other crystals often shows the cleavage plane to have the largest spacing and

therefore the closest degree of packing. This means that the points of the crystal lattice are closest together in that plane, but it does not mean that the atoms or molecules are nearer together in that plane than in any other. Nothing can be said about that until the actual distribution of the atoms in the unit cell has been determined. It would be much safer to say that the existence of a cleavage plane implies a certain *looseness of packing across* the crystal planes which are parallel to the cleavage. This would imply a greater tightness in other directions, but not necessarily a greater proximity. It is only at first glance that the latter term seems to have a clear meaning. But we must let it stand in order to realise the argument as it presented itself to the authors of the statement quoted.

It happens that in the case of bismuth we do actually find a closer bonding between the atoms in the cleavage plane than in any other; but this is peculiar to the structure of bismuth and has no relation to the supposed close proximity of *molecules* in the cleavage plane.

Now we come to the essential point of the argument. It is supposed that proximity offers magnetism or diamagnetism, whichever it may be, the opportunity to "exhibit its greatest energy." We are to remember that the hypothesis on which we are working expresses itself in terms of poles and that a magnet attracts a piece of iron by inducing poles in it, which poles then react with the poles of the magnet. When a piece of iron is allowed to attach itself to a magnet, the poles induced in it are much stronger than if the magnet and the iron are separated by a little distance. If a second piece of iron is brought near the first, every increase in its proximity increases the strength of the poles which are developed in this piece by the influence both of the original magnet and of the first piece of iron.

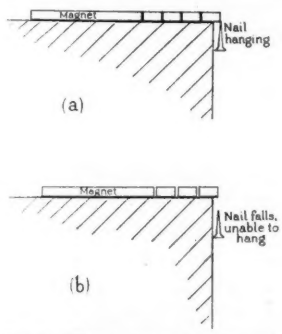


FIG. 8.—When pieces of iron are all in contact with one another and the end one with the magnet, the nail can hang as shown in (*a*). But when, as in (*b*), the iron blocks are somewhat separated from one another and from the magnet, the nail falls.

A simple experiment will serve as an illustration (Fig. 8): The nail hangs in the first case, and not in the second, because the close proximity of the iron blocks increases the strength of the poles in all of them. In the second case, the nail will not hang, although the magnet is actually closer to it. The benefit of mutual 'proximity' of the separate pieces of iron is obvious. An equally simple explanation can be given in terms of lines of force, but we are using the alternative language.

Chains of iron fragments form readily between magnet poles of opposite nature. A rod of iron "transmits the magnetic force," and generally acts more efficiently than a set of iron fragments which are not allowed to get into close proximity with each other. Tyndall sticks short lengths of iron wire through disc-shaped pieces of apple and shows that the disc sets itself at right angles to the field, the bits of wire therefore lying parallel thereto. In each bit are many molecules of iron in close proximity, and the fact is more effective in directing the apple than the existence of a number of bits scattered over the disc without being in 'proximity' to each other.

### Diamagnetism and 'Proximity.'

It is now argued by Tyndall that if the magnetic influence of a magnet is extended by means of proximity, the diamagnetic influence must be extended in the same way. If the close proximity of iron fragments will help them to set with greater firmness in the direction joining opposite poles, then the closer proximity of bismuth fragments should cause them to set with greater firmness across them. In this way Tyndall interpreted the rule, which he believed he had established, that the cleavage planes of magnetic crystals tended to set axially, and those of diamagnetic crystals equatorially.

It is interesting to observe that Tyndall was attempting to supply both a rule for the setting of crystals and an explanation of the rule in terms of structure. Faraday stopped short when he had supplied a picture of the distribution of his magnetic lines, or, as we should now say, a map of the distribution of energy in the magnetic field.

As I have already pointed out, there is no clear meaning to the term "greater proximity in the cleavage plane." Moreover, if conclusions were to be drawn from analogy with phenomena on a larger scale, they would run contrary to the intended argument; for, on that scale at least, a line of diamagnetic masses tends to set itself axially, not equatorially. A piece of bismuth makes an extremely minute alteration in the disposition of the lines of force, for which reason it is a very poor detector of the existence of the lines in comparison with iron; and the change, since it is so small, can indeed be detected by a piece of iron in the form of a magnet, but certainly not by

another piece of bismuth, no matter how close they are together.

In the case of a uniaxial crystal, a principal cleavage must from symmetry considerations be related to the axis and, if it is unique, must be perpendicular thereto. When there is more than one cleavage, the cleavage planes must be symmetrically disposed about the axis as in the case of Iceland spar. It is not, therefore, surprising that, in the former case, the cleavage plane should place itself exactly, either equatorially or axially, and that in the latter case a plane perpendicular to the axis might be looked on as a resultant of cleavage planes and therefore set itself equatorially. But it does seem remarkable that, as Tyndall pointed out, the substitution of iron for calcium in Iceland spar, to form the isomorphous iron carbonate, should turn the structure round through 90° in the magnetic field; especially if we assign diamagnetism and paramagnetism to different causes. It may be there are other cases of the same change; and any rule of this kind must clearly be of importance.

### The Effects of Pressure on Magnetic Susceptibility.

We now come to another set of experiments, very interesting and important, which were used by Tyndall in the defence of the 'polarity' position. The method of these experiments was suggested by an accident. When working in Berlin with a fine magnet placed at his disposal by Magnus, he was observing the action of the magnet on a bismuth cube which was so shaped that two opposite faces were perpendicular to the optic axis and parallel to cleavage planes. When the current was switched on, the magnet poles rushed together because the separate parts of the magnet had not been properly bolted down. The bismuth cube was crushed to some extent. Working conditions having been restored, it was found that the bismuth set itself at right angles to its former position. The line of pressure, which, of course, had been parallel to the field, was now perpendicular to it. Tyndall now argued that the particles of bismuth had been brought into greater proximity by the pressure and that the setting of this line of great proximity was in accordance with the rule given by himself and Knoblauch. So began an extended series of researches on the effects of pressure which are fully described in his book. As an example let us take the following:

"A quantity of bismuth was ground to dust in an agate mortar, gum-water was added, and the mass was kneaded to a stiff paste. This was placed between two glasses and pressed together; from the mass when dried two cubes were taken, the line of compression being perpendicular to two of the faces of each cube and parallel to the other four. Suspended by a silk fibre in the magnetic field, upon closing the circuit the line of compression turned strongly into the equatorial position. . . ."

When carbonate of iron was used the line of pressure set axially.

Such an experiment is very striking, whatever its explanation may be. Tyndall argued that he had by compression increased the proximity along the line of pressure, but it is difficult to see how this can be. If a number of particles of one kind are distributed with complete irregularity in a paste medium which is then subjected to pressure in one direction, the alteration in form of the paste block will not alter the law of distribution of the particles. In any case, as we have already seen, proximity does not produce any observable effects.

### Lord Kelvin's Explanation of Tyndall's Results.

It is surely natural to suggest that the particles acquired some orientation from the pressure, which might well happen if they possessed shapes which were related in some particular way to their structures. Thomson immediately pointed this out to Tyndall, who replied that if that were the case, the bismuth fragments being naturally in the form of flakes parallel to the cleavage plane, the line of pressure ought therefore to set itself axially, whereas it actually set equatorially. This was certainly a good reply. Perhaps the counter argument is that the crystal fragments have not actually been shown to set in this way. Miss Knaggs has made an X-ray measurement of the set of the fragments in one specimen of squeezed dough containing bismuth particles, and has found that the cleavage planes are not closely coplanar with the surface, as they must be if Tyndall's argument is to be good. Though this is a single example, it looks as if a way of escaping the difficulty was to be found.

As I have said, Tyndall's reply to Thomson was good, but, to use his own words, though it formed "a strong presumptive argument it was not yet convincing." He strengthened his case greatly by a further experiment. Comparing the repulsion exerted by a magnet on a natural crystal of bismuth with that exerted on a mass of compressed powder in dough, he found the latter greater than the former. He had cut the crystal into the form of a cube and placed it on one arm of a torsion balance

so that the cleavage plane was perpendicular to the magnetic field, and the repulsive force as great as it could possibly be. The dough had been pressed into a cube of the same size and placed with its line of pressure at right angles to the field. Tyndall argued that there must be a direct effect of pressure, since it had done more than all that the natural phenomena could do.

Now it is clear that if the orientation of a bismuth crystal in a uniform magnetic field, *i.e.* the magne-crystallic action, is due to the arrangement of the atoms and molecules in the crystal structure, the perfect crystal ought to show the effect more perfectly than the fragments distributed through the dough, however perfectly the latter may be arranged. But Miss Knaggs has made an X-ray photograph from the face of a natural 'crystal.' The specimen was chipped out of a mass of crystals left in a crucible, and must have resembled that which Tyndall used. The photograph showed at once that the specimen was a compound of more than one crystal, and that different orientations were present. Cleavage planes, and also others which in a single crystal would make large angles with the cleavage planes, were nearly parallel to the face under test. It is possible, therefore, that there was really more of the effective orientation in the pressed specimen than in the natural piece. A photograph of the single crystal made by Bridgman's method taken in the region of the cleavage plane gave a much cleaner picture.

A piece of bismuth can be looked on as an aggregate of crystals. There may be but one perfect crystal or there may be a number, small or large, of smaller crystals, each perfect. If proximity were increased by pressure, the change in proximity would have to occur in respect to the mutual distances in either of the separate crystals, or of the atoms and molecules in the single crystal. The X-ray analysis shows that the latter alternative is impossible, because from many tests recently carried out in respect to metal structure, we learn that no permanent change in the crystalline lattice is occasioned by stress. The former alternative is also ruled out, because, as Faraday pointed out,[4] bismuth is actually of a lower density after compression than it was before; the pressure having of course been removed. Apparently the breaking up of the specimen increases the extent of the cavities.

Tyndall made many paste models of crystals, mixing powders of bismuth, carbonate of iron, or other active substances with flour and water, or gum. He pressed the mass by different amounts in different directions and then cut it to shape; in this way he imitated the magne-crystallic action in detail. At one time, in order to meet the objection that he was merely rearranging the small crystals in his paste and conglomerates, he took some white wax "concerning whose amorphism there can be but little doubt." The substance is diamagnetic. A little cylinder of the wax suspended in the magnetic field set with its axis equatorial. It was then placed between two stout pieces of glass and squeezed as thin as a sixpence; suspended from its edge, the plate thus formed set so that its length, which coincided with the axis of the previous cylinder, was axial and its shortest dimension equatorial. But we know now that wax is anything but amorphous; its crystalline structure has not only been observed but also accurately measured; and we know also that pressure arranges the orientation of the crystals.

Tyndall obtained the same result with a piece of bread, and we may repeat the experiment. A small piece of the crumb is squeezed between two glass plates, and the edges of the irregular mass are trimmed off, so as to leave a thin disc. When this is suspended so that its plane is vertical, it sets equatorially if the poles are close together and the field is very divergent. It is therefore diamagnetic. But when the bread is moved from the space between the poles to a more uniform part of the field, the plane of the disc turns through a right angle and sets itself parallel to the lines of force. It is quaint to observe how the bread, as it is moved up to the poles, sets itself to pass neatly through the narrow gate and take up a parallel position on the further side. This is due to magne-crystallic action, so that the bread contains crystals, a fact easily verified by X-ray methods.

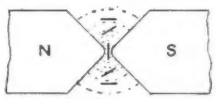


FIG. 9.—The black lines show different positions of a thin wafer of pressed bread hung by a single fibre. In the outer parts of the magnetic field it sets more or less along the lines, but as it is brought up to the more intense parts, where there is great divergence of the lines, it turns so as to set itself at right angles to the field.

### The Effects of Pressure on Crystalline Condition.

The long series of interesting and ingenious experiments which Tyndall made to show that pressure produced proximity and proximity produced the equivalent of magne-crystallic action, must be held to have failed in their original purpose. But they will doubtless be put to a different use.

[4] His reference was to "Gmelin's Handbook of Inorganic Chemistry," vol. 4, p. 428.

They are related to a subject of immense importance in these days, namely, the effects of pressure and tension and mechanical treatment generally, upon the state of a material and upon its physical properties. The consideration of such questions is fundamental to metallurgy and to other industries. The microscope has for many years been employed for the purpose, and the new methods of X-ray analysis are already being put into service. It may well repay us to consider Tyndall's experiments in a new light; and to examine the actual nature of those rearrangements which produced such remarkable changes in magnetic reactions. Tyndall himself discussed the effects of pressure in producing planes of possible cleavage, and was one of the pioneers in showing how such planes, occurring in the earth's crust, were not always to be interpreted as the result of sedimentary deposition, but rather of pressure, which might, if it were exerted more or less along the deposition planes, produce cleavages across the latter. He extended the principle to account for stratification in rolled materials, even in biscuits and pastry!

Faraday's use of lines of force did not, in reality, demand so much framing of hypothesis as Tyndall's polarity. It is to be observed that, as Faraday pointed out, they had no differences about facts, merely about methods of description, which methods, however, were of different value as suggesting development. To Faraday's conceptions have been added theories of magnetism and diamagnetism based on the existence of resistanceless molecular circuits as imagined by Ampère and Weber, or on revolving electrons as explained by Langevin. In the most recent times the quantum theories have again modified our ideas.

### Modern Considerations.

The crude hypothesis of the molecular circuit leads simply to a useful point of view of the difference between paramagnetism and diamagnetism, and the most modern discussions, though they differ greatly in appearance, leave that point of view almost untouched. If any of Faraday's lines of force thread a circuit which has no electrical resistance, that number can never be changed. If, therefore, a substance be brought into a magnetic field, the molecular circuits in the atoms of the substance act like obstructions to the lines; and the total obstruction, of which the negative magnetic susceptibility is a measure, is proportional to the sum of the areas of all these circuits, as projected on a plane perpendicular to the lines. It is of no consequence whatever whether there are already currents in those circuits; unless, indeed, the circuits are movable and can alter their set towards the imposed field. Thus the diamagnetism is unaffected by the existence of molecular magnetic fields; or by any changes in them, so long as the total of the projected areas of the circuits is unchanged.

This result does not hold if circuits approach each other so closely that they offer less obstruction to the lines than if they were more separated. Two resistanceless circuits running closely parallel to each other offer little more opposition to the passage of lines than either circuit alone. We should imagine that such changes in the relative position of circuits would only occur in strenuous circumstances such as, possibly, those of crystallisation. It is known that diamagnetic susceptibility may vary very slightly: for example, Oxley has shown that crystallisation sometimes brings about small but definite alterations. As has often been pointed out, this simple theory makes diamagnetism a property of all substances, which can be affected, even overwhelmed, when the circuits already contain currents, and therefore can be orientated afresh by the magnetic field.

Let me say in conclusion that although recently acquired knowledge of the structure of materials leads us to reconsider Tyndall's experimental results, we are still far from the full explanation of the connexion between structure and magnetism, and of the influence of the latter upon physical properties.

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## The Prediction of Eclipses.

By Dr. L. J. COMRIE.

THE fact that astronomers can predict, many years in advance, the time of a solar eclipse to within a few seconds, and the region on the earth's surface in which it will be visible to within a mile or so, will perhaps surprise the layman. Yet attention was directed to the forthcoming eclipse of June 29 more than half a century ago by the Rev. S. J. Johnson in his "Eclipses Past and Future."

These advance predictions are possible because eclipses pass through a definite cycle, or repeat themselves after an interval of 18 years $11\frac{1}{3}$ days. To explain the reason for this, consider the conditions that lead to an eclipse. It is well known that the earth moves round the sun in a nearly fixed plane called the ecliptic. As seen from the earth the apparent motion of the sun in the heavens is along the great circle which represents the intersection of the ecliptic plane and the celestial sphere, or the ecliptic circle. If the motion of the moon round the earth were in the same plane, then once in every lunar month of $29\frac{1}{2}$ days the moon would be in line with the sun, and an eclipse would result. Actually the plane of the moon's orbit is inclined to the plane of the ecliptic at an angle of 5°. This means that the great circle representing the moon's orbit crosses the ecliptic at two opposite points or nodes, while half-way between the nodes the nearest possible approach of the two bodies is 5°.

Should the sun happen to be within a certain limiting distance from either of these nodes at the time the moon crosses the ecliptic, there will be an eclipse, either partial or total. If the nodes remained fixed on the ecliptic, eclipses would take place at the same two seasons each year. Actually the nodes make a complete circuit of the ecliptic in a backward or retrograde direction in a little less than nineteen years, so that the sun, in its annual course through the ecliptic, returns to the same node, not in 365·24 days, but in 346·62 days, a period called the eclipse year. Nineteen of these periods equal 6585·78 days. The interval between successive conjunctions of the sun and moon, or, in other words, the interval between successive new moons, is called the synodical month, and is 29·53 days. 223 of these lunations contain 6585·32 days. That is to say, if the sun and moon at any given moment are in conjunction at or near a node, so that an eclipse is in progress, then after 6585·32 days they will again be in conjunction and in the immediate neighbourhood of a node, so that another eclipse will occur.

The circumstances of an eclipse vary considerably with the distance of the moon from the earth—from an annular eclipse when the moon is at its maximum distance of about 257,000 miles to a long total eclipse of perhaps six or seven minutes' duration when the distance is as small as 223,000 miles. The interval between successive returns to the same distance from the earth, or the anomalistic month, is 27·55 days, and 239 of these intervals are 6585·54 days. Thus not only is there a repetition of the eclipse after 6585 days, but also it will occur under practically the same conditions.

This most useful period of recurrence was known to the Chaldeans as the Saros, and formed the basis of their very successful eclipse predictions. It is still used for the purpose of fixing the dates on which eclipses will occur, although the details of the eclipses are obtained by more refined methods.

One question must be answered. Why was not a total eclipse visible in England eighteen years ago, in 1909? The reason is that 223 lunations exceed 6585 days by about 8 hours, and in that time the earth has rotated, so that successive corresponding eclipses occur, on the average, 120° of longitude farther to the west.

Mention should be made here of Oppolzer's celebrated "Canon der Finsternisse" (Vienna, 1887), which gives particulars of all eclipses between 1207 B.C. and A.D. 2161, together with maps showing the central lines of total eclipses. An inspection of these maps indicates that the eclipse of 1999 will be total in Cornwall only, and that the next total eclipse visible in England in 2135 is the first of a group of four that will be seen in

the course of 25 years, namely, in 2135, 2142, 2151, and 2160.

The accurate prediction of a solar eclipse is dependent ultimately on accurate predictions of the positions of the sun and moon. These bodies have been carefully observed with meridian circles for more than two centuries. The principal object in founding the Royal Observatory at Greenwich was the making of these observations, and to this day unrelaxing efforts are made to observe every meridian passage. In the hands of those masters of celestial mechanics, Delaunay, Hansen, Leverrier, Newcomb, Hill, and Brown, these observations and Newton's gravitational theory have led to tables from which the positions of the sun and moon for any date in historical times, or for centuries to come, may be found. Even the eclipses recorded by the ancients have contributed to these tables, for it is evident that the tables should reproduce the eclipses as observed.

The tables used by the "Nautical Almanac" are Newcomb's "Tables of the Sun" and Brown's "Tables of the Moon." The latter is a ponderous tome, too heavy, when bound, to be accepted by the British Post Office! Its two million figures, printed at Cambridge for the Oxford University Press, are the life-work of an English-born professor living in the United States. It requires the continuous efforts of two highly skilled computers to produce from these tables the hourly ephemeris of the moon given annually in the "Nautical Almanac."

The problem of predicting the circumstances of an eclipse for a given point on a non-spherical rotating earth would at first sight seem hopelessly difficult. But the classical solution offered a century ago by Bessel, and later most ably expounded by Chauvenet, has, by the simplicity, beauty, and rigour of its conceptions, lived to this day.

The movements of the sun and moon are expressed by means of suitable rectangular co-ordinates on a fundamental plane through the centre of the earth, and at right angles to the line joining their centres. The shadow of the moon is a cone, the intersection of which with the fundamental plane is a circle. Upon this same plane the position of the observer is projected orthographically, and the projected distance from the origin, which is the centre of the earth, is resolved into components parallel to the previously chosen axes.

The co-ordinates $x$ and $y$ of the moon's centre, which are also the co-ordinates of the centre of the shadow, and those of the observer $\xi$ and $\eta$, will be the same on a plane through the observer and parallel to the fundamental plane, but the radius of the shadow-cone, $L$, which is readily determined, will be different. The fundamental equation of eclipse prediction simply expresses the condition that when an eclipse is beginning or ending the observer is on the edge of the shadow cone, or his distance from the centre of the shadow cone is equal to its radius. Symbolically,

$$(x-\xi)^2+(y-\eta)^2=L^2.$$

The two times when this quadratic equation is satisfied represent the beginning and ending of an eclipse.

The quantities $x$ and $y$, the dimensions of the shadow cone, and other functions which are independent of the position of the observer, are tabulated in the "Nautical Almanac" for each eclipse as Besselian elements. With the aid of these elements, complete predictions for any given place can be made in a few hours.

The difficulties which prevent the making of perfect predictions must now be reviewed. First, the diameters of the sun and moon. When a bright body is projected on a dark background it appears to be larger than its true size—a phenomenon known as irradiation. Hence the diameter of the moon as usually measured has to be considerably reduced for eclipse purposes; in fact, the so-called eclipse diameter, which is used in predictions, has been determined from eclipse observations alone. Further, the limb of the moon is irregular, owing to the presence of lunar mountains; on this account alone an exact prediction cannot be made, for a valley 1000 feet deep could affect the time of eclipse by a second or more, especially if the observer were near the northern or southern limit of totality.

Another difficulty lies in the unavoidable errors of the solar and lunar tables. These arise partly from the fact that some of the quantities required in their construction, such as, for example, the masses of perturbing planets, are exceedingly difficult to determine, even from a prolonged series of observations. Another contributory cause is the fact that there appear to be some unknown influences at work. Prof. E. W. Brown, formerly a pupil of the illustrious George Darwin, in the preface to his "Tables of the Moon," says: "While many efforts have been made in the past to represent the motion of the moon by gravitational theory alone, it is now admitted that this cannot be done completely. . . . There are oscil-

lating differences which do not correspond to any theoretical gravitational terms. . . . The causes of these differences . . . are matters of conjecture.[1] . . . Still more puzzling are certain oscillations with smaller amplitudes and shorter periods. . . . All that can be done is to make an estimate . . . from the observations of the past few years whenever it is desirable to predict the position of the moon with high accuracy, as in the case of an eclipse of the sun, and alter the values obtained from the Tables accordingly."

When the coming eclipse was first accurately predicted three years ago, a correction of +7″·0 was applied to the mean longitude of the moon as derived from Brown's "Tables," but, strangely, no correction was applied to the position of the sun. The Astronomer Royal, Sir Frank Dyson, has quoted the corrections to the longitude of the sun and moon derived from recent Greenwich observations as +1″·5 and +6″·5 respectively. When these corrections replace those formerly used, the effect is very slight; it amounts to a displacement of the central line and the zone of totality as shown on the Ordnance Survey Eclipse Map, the data for which were computed from the original elements, by just one mile in a north-westerly direction.

[1] Since the above was written, in 1918, several astronomers have suggested that the earth's period of revolution is variable, and have adduced evidence of a correlation between the anomalies in the motions of the Sun, Moon, Mercury, Venus, and Mars.

The residual uncertainty, after the application of these corrections, should be less than half a mile in the case of the central line, and not more than a mile in the case of the northern and southern limits of totality.

The co-ordinates of the central line, and the circumstances of the eclipse along this line, are given in the table below:

| G.M.T. | Longitude. | Latitude. | Sun's Altitude. | Sun's Azimuth. | Duration. |
|---|---|---|---|---|---|
| | ° ′ | ° ′ | ° | ° | s |
| 5h 23m 0s | +4 55·0 | 52 32·1 | 9·8 | 64·2 | 20·9 |
| 10 | 4 35·6 | 52 43·3 | 10·1 | 64·5 | 21·2 |
| 20 | 4 16·7 | 52 54·3 | 10·3 | 64·8 | 21·5 |
| 30 | 3 58·3 | 53 5·0 | 10·6 | 65·1 | 21·7 |
| 40 | 3 40·3 | 53 15·5 | 10·9 | 65·4 | 22·0 |
| 50 | 3 22·8 | 53 25·8 | 11·1 | 65·7 | 22·3 |
| 5 24 0 | +3 5·7 | 53 35·8 | 11·4 | 66·0 | 22·6 |
| 10 | 2 48·9 | 53 45·7 | 11·6 | 66·3 | 22·9 |
| 20 | 2 32·5 | 53 55·4 | 11·8 | 66·5 | 23·2 |
| 30 | 2 16·4 | 54 4·9 | 12·1 | 66·8 | 23·5 |
| 40 | 2 0·6 | 54 14·2 | 12·3 | 67·1 | 23·7 |
| 50 | 1 45·1 | 54 23·4 | 12·5 | 67·3 | 23·9 |
| 5 25 0 | +1 29·8 | 54 32·5 | 12·7 | 67·6 | 24·2 |
| 10 | 1 14·8 | 54 41·5 | 12·9 | 67·8 | 24·4 |
| 20 | 1 0·0 | 54 50·3 | 13·2 | 68·1 | 24·6 |
| 5 25 30 | +0 45·5 | 54 59·0 | 13·4 | 68·3 | 24·9 |

The co-ordinates of the central line, and the circumstances of the eclipse along this line, are given in the table below:

## The Recurrence of Solar Eclipses.

By Dr. J. Jackson.

AMONGST the most remarkable of discoveries made by ancient astronomers was that of the recurrence of eclipses at intervals of 18 years and 10 or 11 days. We have no knowledge of the discoverer of this period, known as the Saros, but it was certainly known to the Chaldeans. In view of the irregularities of the early calendar, such a discovery must have presented great difficulties. The fact that the interval has an odd third of a day, so that the region of visibility of an eclipse is shifted about 120° in longitude at each return, greatly increases the difficulties of discovery, and it is possible that a period three times as long as the Saros was first discovered. As the area of the earth from which an eclipse can be seen extends over a large arc in longitude, it is possible for two consecutive members of a series of eclipses to be seen from the same place. The total eclipse visible in England on Aug. 11, 1999, is indeed four Saroses later than that of June 29 of this year, but whereas this year's eclipse is in the early morning, the eclipse of Aug. 11, 1999, will be visible in England shortly before noon.

The circumstances connected with the recurrence of eclipses depend on several variables with different periods, and the apparent irregularity with which eclipses occur results from the incommensurability of the periods and differences in their relative importance. The most important period is that between successive new moons, which on the average is 29·5306 days. An eclipse of the sun would take place at every new moon if the orbital planes of the sun and moon coincided,[1] but as the inclination of the two planes is considerable—varying round 5°—it is only when new moon occurs near the line of intersection of the two planes, known as the line of nodes, that an eclipse can take place. On account of the motion of the plane of the moon's motion, the sun passes through the nodes at intervals of less than six months, the average time being 173·310 days, and this is the second important period in connexion with eclipses. Eclipses take place at intervals which are very nearly multiples of 29·5306 days and are approximately multiples of 173·310 days.

The maximum angular distance which the sun

[1] If this were the case, however, all central eclipses would take place within the tropics, and the only eclipses that could be seen from England would be extremely small partial eclipses at the new moons near midsummer.

can be from the node at the time of an eclipse (known as the eclipse limit) depends on the distances of the sun and moon from the earth and the actual inclination of the orbital planes. It varies from $15\frac{1}{3}°$ to $18\frac{1}{2}°$. As the sun moves on the average almost exactly $2 \times 15\frac{1}{3}°$ relative to the nodes between successive new moons, and as it is moving most slowly when the eclipse limits are smallest, an eclipse occurs near every passage of the sun through a node. There are thus at least two solar eclipses every year, and there may be as many as five, the latter only occurring when the sun passes through a node near the beginning or end of the calendar year. Eclipses of the moon may take place at the full moons preceding and following eclipses of the sun, although for them the eclipse limits are smaller.

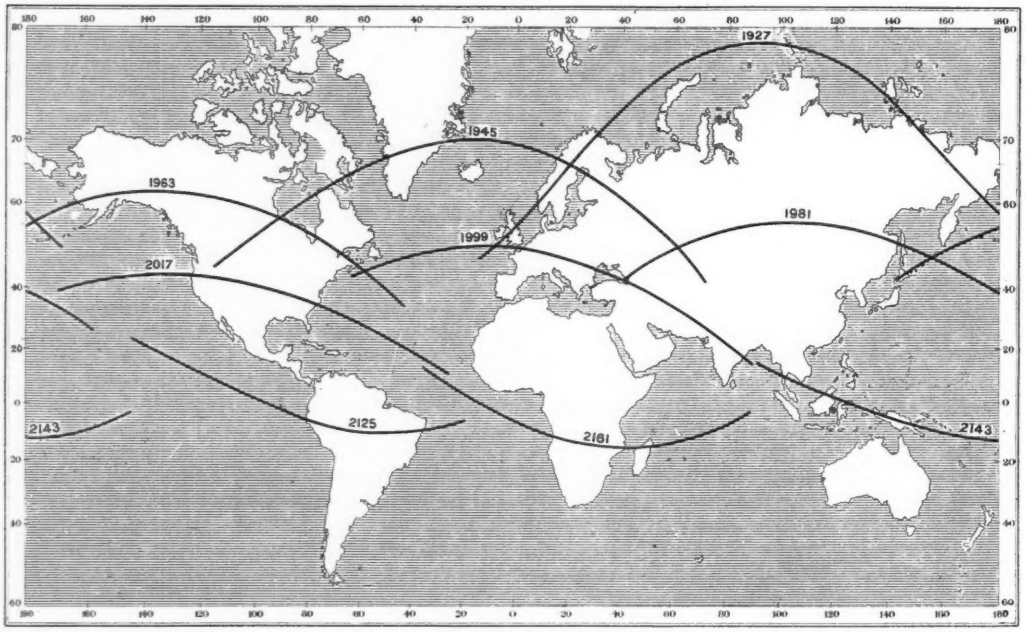


FIG. 1.—Successive tracks of solar eclipses.

The Saros is connected with the two periods we have just mentioned. We have in fact

$$223 \times 29{\cdot}5306 \text{ days} = 6585{\cdot}32 \text{ days.}$$
$$38 \times 173{\cdot}310 \quad \text{,,} \quad = 6585{\cdot}78 \quad \text{,,}$$

The Saros is 6585·32 days. As the sun travels on the average about 1° a day, it describes in a Saros only about 28′ less than 19 revolutions relative to the nodes. As 28′ is a small fraction of the eclipse limits, we find a large number of eclipses recurring in a series.

The number of eclipses in a series varies one or two either way from seventy and extends over a period of 1200 years. Of these, about twelve or thirteen at each end are only partial eclipses, as the sun is so far from the node that the line going through the centres of the sun and moon passes clear of the earth. For the middle, forty-five or so, this line comes to earth, giving rise to a central eclipse which is total or annular according as the angular diameter of the moon or sun is the larger.

The eccentricity of the apparent orbit of the sun is small, so that the angular semi-diameter of the sun varies only from 15′ 46″ to 16′ 18″. Also we have seen the Saros differs from an exact number of years by about 11 days, so that at the recurrence of an eclipse the apparent diameter of the sun cannot have altered more than 3″. The eccentricity of the lunar orbit is larger, causing the apparent semi-diameter of the moon to vary from 14′ 42″ to 16′ 46″. On account of the rather rapid motion of the moon's apse, amounting to a whole revolution in about nine years, the interval between successive nearest approaches of the moon to the earth is on the average 27·55455 days and

$$239 \times 27{\cdot}55455 \text{ days} = 6585{\cdot}54 \text{ days.}$$

This differs from the Saros by only 0·22 of a day. This is a remarkable and important coincidence. It means that after a Saros the mean anomaly of the moon has altered by only about 2°·8 (decrease), and the apparent diameter of the moon, like the sun, has not altered by more than 3″. The con-

sequence of all this is that the duration of totality in a series of eclipses varies slowly from one eclipse to the next, and that we have a series of total eclipses, or of annular eclipses. If the period of rotation of the lunar apse were twelve years instead of nine years, annular and total eclipses would alternate. It might be noted here that as a result of the moon returning to approximately the same distance from the earth after a Saros, its parallax is only slightly altered, and so the eclipse limits only slightly altered.

Let us now consider the series of eclipses to which that of June 29 of this year belongs. At the new moon of May 26, 1873, the central line from the sun to the moon passed just north of the earth and there was a large partial eclipse near the north pole. If we work backwards from this date at intervals of 18 years and 10 or 11 days, we find a series of decreasing partial eclipses near the north pole. Going forwards, we find that on June 6, 1891, the central line passed over the north pole but came to earth in northern Asia, producing an eclipse at midnight. This was an annular eclipse very nearly total, and was the first central eclipse of the series to which the eclipse of this year belongs. Eighteen years later, on June 17, 1909, there was an eclipse which crossed the earth near the pole. This eclipse was just total, although it was scarcely certain beforehand that it would be so. We then come to the total eclipse of this year. Proceeding onwards, we get a series of eclipses gradually lengthening in duration and working equatorwards, while the longitude on the earth where the eclipse takes place moves about 120° westward from each eclipse to the next.

The "Nautical Almanac" gave the semi-diameters of the sun and moon as seen from the centre of the earth for the eclipses of 1891, 1909, and 1927 as follows:

| | Moon. | Sun. |
|---|---|---|
| 1891 June 6 . . | 15′ 42″·2 | 15′ 47″·5 |
| 1909 June 17 . . | 43″·1 | 44″·3 |
| 1927 June 29 . . | 46″·7 | 44″·0 |

These figures indicate the way in which the apparent diameter of the moon is gradually increasing relative to that of the sun. Also on the eclipse track the apparent diameter of the moon must be larger than from the earth's centre, so that the total eclipses of this series will be steadily lengthening.

The following table indicates the change of position of the point on the earth where central eclipse occurs at noon, for a few eclipses belonging to the series of the eclipse of this year. The longitudes are measured towards the east. It will be seen that the track moves about 8° southward each year and a little more than 100° in longitude. After two hundred years, when the track is near the equator, the movement is about 5° southward and 120° westward each year.

| | Longitude E. | Latitude. |
|---|---|---|
| 1909 June 17 . . | 187° | +88° |
| 1927 June 29 . . | 84 | +78 |
| 1945 July 9 . . | 340 | +70 |
| 1963 July 20 . . | 234 | +62 |
| 1981 July 31 . . | 127 | +54 |
| 1999 Aug. 11 . . | 18 | +46 |
| ..... | ..... | ..... |
| 2107 Oct. 16 . . | 39 | + 2 |
| 2125 Oct. 26 . . | 276 | − 4 |
| 2145 Nov. 7 . . | 150 | − 9 |
| 2161 Nov. 17 . . | 23 | −14 |

The map (Fig. 1) shows the track of the approximate central line for the eclipses of 1927, 1945, 1963, 1981, 1999, 2017, and 2125, 2143, 2161. In each case the eclipse begins at the western end of its track at sunrise and finishes at the eastern end at sunset.

## Future Total Solar Eclipses in the British Isles.

By Dr. A. C. D. Crommelin.

An article by Dr. W. J. S. Lockyer in Nature for Jan. 15 last described and illustrated the total solar eclipses in the British Isles from A.D. 878 to A.D. 1999. The present article continues this investigation for another thousand years. In view of the very long interval of two centuries that has elapsed since there has been a British totality, it is interesting to determine the average interval between these events. Mr. J. Maguire's list, used by Dr. Lockyer, has a few omissions. I therefore had recourse to the maps in Oppolzer's Canon of Eclipses. These indicate 63 British totalities in 3370 years (1208 B.C. to A.D. 2161), or 19 in a thousand years, giving an average interval of 54 years between totalities. I am aware that the maps in the Canon show the tracks as circular arcs not quite agreeing with the true ones, but for statistical purposes this is of no importance; we clearly gain as many as we lose by the distortion.

As a check I find that Otto Schrader gives 21 eclipses as total in the British Isles in the period A.D. 2133 to A.D. 3045. Four of these are doubtful; the central line lying outside these islands: giving

these half weight, we have 19 in 900 years, or 21 in a thousand years. So we are safe in concluding that the average number in a thousand years is 19 or 20, giving an average interval of 53 years. I have here included the Shetlands in the British Isles. If we exclude them (they were not included in Dr. Lockyer's map, reproduced in Fig. 1) we may take the average interval as about 58 years.

If instead of a large area we consider a point on the earth's surface, we have on the average 3 total eclipses, 4 annular ones, and nearly 400 partial ones visible there in 1000 years. But we should have to take several thousands of years to make the average numbers for totality and annularity correct. In shorter periods we find some striking anomalies; thus a small region in Scotland round the point 3° 40′ W., 55° 30′ N., will have three totalities all under favourable conditions within 61 years (A.D. 2381, 2426, and 2442). Also, the grouping of totalities in the British Isles as a whole is much less regular than we might expect as a result of three such uniform motions as the rotation and revolution of the earth and the revolution of the moon. They bunch at one epoch and spread out at another in a manner that recalls the service of tram-cars. The blank period of 203 years that is now ending is the longest between A.D. 800 and A.D. 3100. The gap between A.D. 2200 and A.D. 2381 comes second, and that between A.D. 1433 and A.D. 1598 third.

On the other hand, the most striking example of bunching is between A.D. 2081 and A.D. 2200; we have here seven certain totalities and two others that are likely to reach the shores of Great Britain within 119 years.

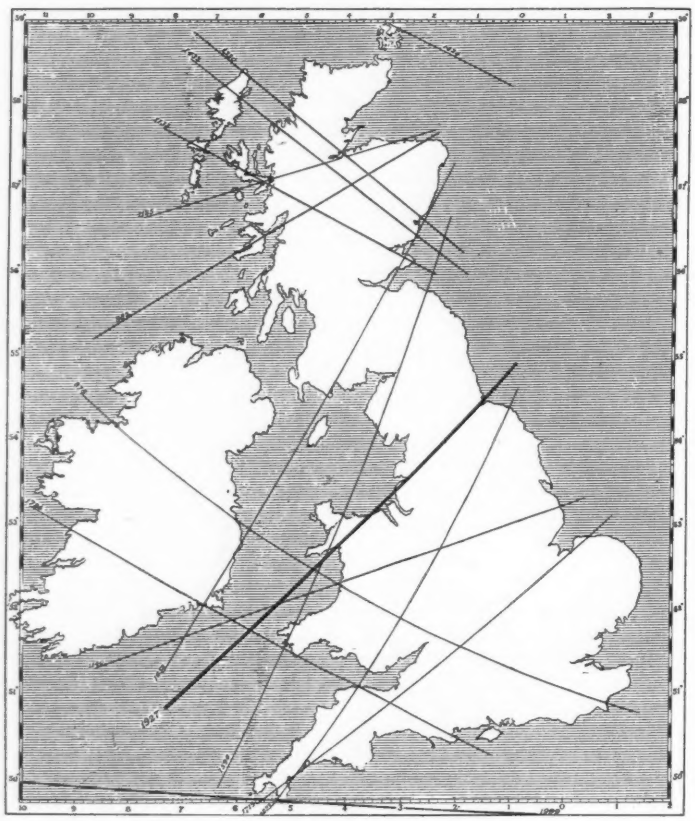

FIG. 1.

The following are the data from which the tracks in the accompanying map (Fig. 2) were laid down:

(1) For the period A.D. 2000 to A.D. 2161 they are from my own calculations, using the elements in Oppolzer's Canon.

(2) For A.D. 2189 from my own calculations, using Hansen's tables of the moon and Newcomb's of the sun. This eclipse was inadvertently omitted by Schrader.

(3) From A.D. 2200 to the end from Otto Schrader, "Die bedeutenden Sonnenfinsternisse und die grossen Mondfinsternisse für Mittel-Europa" (P. Stankiewicz, Berlin, 1913).

It is to be remembered that the lunar tables used by Oppolzer omit many small terms; consequently the eclipse tracks calculated from them are uncertain by about 25 miles at the present time; and since the accelerations used in them differ considerably from those now accepted, the uncertainty becomes markedly greater after an interval of several centuries either in the past or the future. I understand that the tables used by Schrader are simply an extension of Oppolzer's, so they are subject to similar uncertainty, and tracks several centuries ahead can only be taken as approximate.

A few notes follow on some of the eclipses the central lines of which are shown on the map. The eclipse of A.D. 2081 is a return after seven Saroses of the Philippine Islands' eclipse of 1955, which has the remarkable duration of totality of $7^m\ 6^s$. If Oppolzer's data are accurate totality should reach the Lizard. Although the central line in A.D. 2090 is some distance from the shores of the British Isles, the track is so wide through foreshortening

that the shadow will reach the south-west coast. In A.D. 2142, totality may reach Dungeness. In A.D. 2151, Oppolzer's elements would give totality in London, but those of Hind, Maguire, and Johnson do not; their tracks are farther north. The eclipses of A.D. 2381, 2681, and 2726 are remarkable for long totalities. The latter seems to be a record for Europe, reaching 6 minutes. The Lizard will probably be within the shadow; the Channel Islands certainly will.

The following table gives the dates of all total eclipses that may reach the shores of the British Isles, the approximate hour, the sun's altitude and the duration on the central line. I have diminished the durations given by Oppolzer's and Schrader's elements by $0{\cdot}2^m$, since they use too large a diameter for the moon.

| Date. | Hour. | Sun's alt. | Dur. |
|---|---|---|---|
| 2081 Sept. 3 | 7 A.M. | 18° | $3{\cdot}5^m$ |
| 2090 Sept. 23 | 5 P.M. | 6 | 2·8 |
| 2133 June 3 | 9 A.M. | 40 | 3·5 |
| 2135 Oct. 7 | 8 A.M. | 7 | 2·7 |
| 2142 May 25 | 9 A.M. | 43 | 2·9 |
| 2151 June 14 | 6 P.M. | 15 | 2·6 |
| 2160 June 4 | 6 P.M. | 22 | 2·1 |
| 2189 Nov. 8 | 8 A.M. | 8 | 2·5 |
| 2200 April 14 | 5 P.M. | 17 | 0·4 |
| 2381 July 22 | 10 A.M. | 48 | 5·0 |
| 2426 Sept. 2 | 8 A.M. | 25 | 3·9 |
| 2442 April 11 | 9 A.M. | 33 | 1·7 |
| 2545 April 12 | 5 P.M. | 15 | 0·2 |
| 2600 May 5 | 6 A.M. | 12 | 2·7 |
| 2681 June 8 | 2 P.M. | 50 | 4·5 |
| 2726 July 21 | 11 A.M. | 58 | 5·7 |
| 2808 Aug. 13 | 6 A.M. | 8 | 0·3 |
| 2817 Sept. 2 | 4 P.M. | 21 | 0·9 |
| 2864 Feb. 28 | 1 P.M. | 29 | 2·8 |
| 2911 Aug. 15 | 2 P.M. | 37 | 3·0 |
| 2927 Mar. 24 | 2 P.M. | 31 | 3·0 |
| 2972 May 4 | 3 P.M. | 37 | 3·8 |
| 2974 Sept. 7 | 1 P.M. | 35 | 4·1 |

The central lines of 2081, 2090, 2726 pass respectively 70, 140, and 93 miles south of the Lizard; that of 2974 passes 44 miles north-east of Unst (Shetlands).

In the above list the eclipses of A.D. 2081, 2135, and 2189 illustrate the triple Saros; those of A.D. 2133, 2151, and 2142, 2160 the simple Saros; those of A.D. 2864, 2972 the sixfold Saros: the intermediate eclipse in 2918 passes north of the Shetlands. Those of A.D. 2081, 2381, and 2681, also A.D. 2142, 2442, also A.D. 2426, 2726, illustrate the 300-year cycle: those of A.D. 2160, 2681 illustrate the 521-year cycle.

Taking the present list and Dr. Lockyer's together, there are two examples of a 2-year interval between British totalities: A.D. 2133, 2135, and A.D. 2972, 2974; three of a 7-year interval: A.D. 878, 885, and A.D. 1133, 1140, and A.D. 2135, 2142; six of a 9-year interval: A.D. 1424, 1433, and A.D. 1715, 1724, and A.D. 2081, 2090, and A.D. 2133, 2142, and A.D. 2151, 2160, and A.D. 2808, 2817.

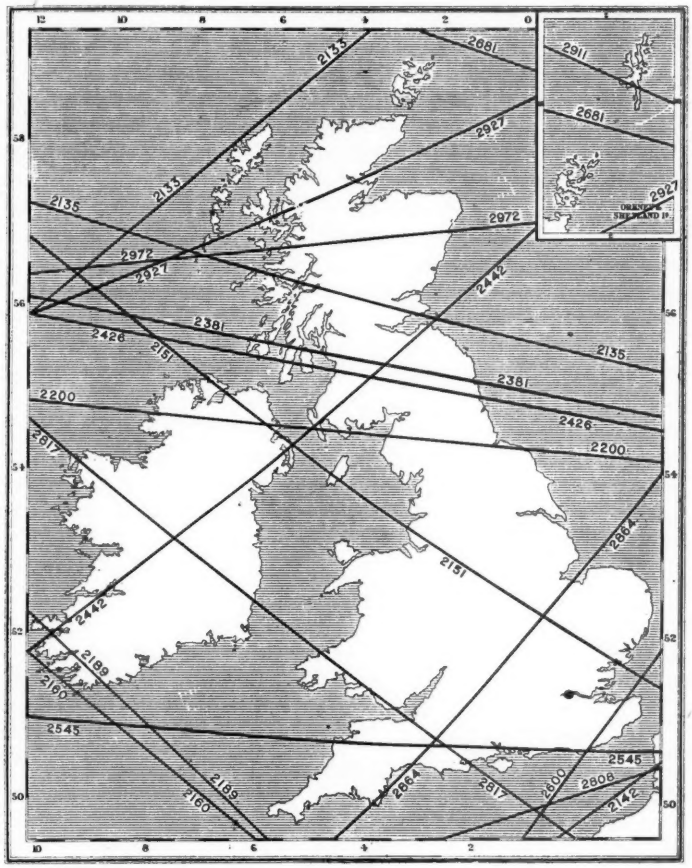


FIG. 2.

## Meteorological Conditions in Relation to Eclipse Observations.

By R. CORLESS.

THE eclipse of the sun which will occur at about 5 h. 23 m. G.M.T. (6.23 A.M. summer time) on June 29 will appear total to observers stationed upon a strip of country 28 or 29 miles wide, of which the central line stretches from Criccieth in North Wales to Hartlepool in Durham. The character of the country included in this strip is very varied. There are coastal areas on the Lleyn Peninsula and North Wales, in Lancashire and between Sunderland and Salt-

burn on the east coast; there are, on the other hand, the mountain peaks of Snowdon, Whernside, Penyghent, and many others; and there is a considerable gradation of hill, plateau, valley, and plain between these extremes.

It is worth consideration whether the climatological records suffice to indicate which type of country in the belt is likely to provide the most favourable weather conditions for viewing the eclipse. The question resolves itself into the identification of the places which, on the average, are most free from cloud, mist, and fog. The records do not furnish a direct answer to this question, because observations of cloud are not sufficiently numerous and well distributed along the belt to define with precision the average distribution of cloudiness. A consideration of other records, notably of rainfall, sunshine, and visibility, indicates, however, that the conditions which produce bad seeing are so different in character on different occasions, that it is better not to deal with averages, but to classify the various occasions into types, and to consider the physical conditions associated with different types.

A broad classification can be made into the following occasions:

(1) When seeing is bad everywhere.
(2) When it is variable.
(3) When it is bad on the hills and good elsewhere.
(4) When it is bad in the low-lying valleys and good elsewhere.
(5) When it is good everywhere.

(1) Occasions when seeing is bad everywhere are most likely to occur when a widespread canopy of cloud is produced in a depression or region of relatively low barometric pressure. But they also occur in certain anticyclones where an inversion of temperature at a moderate height prevents vertical motion of air, and so favours the development of a layer of cloud just below the inversion. In the latter case the summits of mountains may be places from which a good view can be obtained.

(2) The condition in which seeing is variable and changeable is characteristic of a depression of less intensity than that of the previous case. Especially in the rear of the disturbance the weather may be showery, and bands of cloud may alternate with stretches of relatively clear sky.

(3) The third case, in which seeing is bad on the hills and good elsewhere, is in general due to the formation of persistent cloud on and above the hills as a result of the lowering of temperature in a current of rather moist air, which is forced upwards by the obstacle of the hills themselves. The clouds disappear on the lee side of the hills because the temperature of the air-current increases again in its descent after surmounting the crest. In this case a definite air-current is an essential part of the process, and as wind is detrimental to fog formation, mist and fog are not normally found at low levels.

(4) In this case seeing is bad in the low-lying valleys, in consequence of the development of mist or fog, but good elsewhere. This condition is characteristic of the early morning in anticyclonic, cloudless summer weather. It arises from the cooling of the ground by radiation to the clear sky during the previous night, which leads to the cooling of the surface layers of air by conduction. Conditions being anticyclonic, there is usually little or no wind, consequently the cold surface air flows into the valleys by gravitation. The temperature of this air continues to fall as the night progresses, until by daylight it is probably below the dew-point, and the cold air is coterminous with a sea of fog with a flat, well-marked top, which is fatal to good seeing for observers enveloped in it. Above the fog the air is clear and cloud is usually absent.

(5) The fifth case provides the most favourable conditions for good visibility everywhere. It differs from the last in that fog is not formed in the valleys because air movement in the anticyclone is sufficient to overcome the tendency of the chilled surface layers to gravitate into the valleys. Mixing of the lower layers by turbulence takes place and prevents the temperature from falling to the dew-point. Meteorologically the two cases are not readily distinguished, but the results are very different to observers stationed in deep valleys.

In the absence of information as to the probable distribution of barometric pressure on the morning of the eclipse, the only practical conclusion that can be drawn is that it is advisable to avoid both high ground and very low ground. But on the day prior to the eclipse it should be possible, with the aid of the current weather map, to form an opinion as to which of the five general conditions of weather described above is likely to be experienced on the following morning.

## Naked-Eye Observations of an Eclipse of the Sun.

By Annie S. D. Maunder.

NO observer of the partial phase of a total solar eclipse should look at the sun with the naked eye. During this phase some people advise the use of coloured gelatine spectacles, smoked glass, or darkened photographic film; others advocate a graduated screen, using always the densest possible part of it. A very serious attitude is taken in the *Lancet* of April 30, where two ophthalmic surgeons urge that the public must be made to understand that neither smoked glass nor fogged photographic film are safe screens even when well prepared, seeing that they do not absorb the irritant ultra-violet rays, and to avoid the risk of 'eclipse blindness' use must be made of a photographic film which (after fogging so as to obtain the necessary density) has been treated by a special process which excludes wave-lengths shorter than 4000 Å.U.

My own precautionary method is at once safer, cheaper, and more effective. It is this: Do not look at the sun at all—either with the eyes screened or unscreened—until totality is just about to begin. During the partial phase there is nothing on the sun of interest that cannot be seen better by looking on the earth. In other words, project in all cases the sun's image on a screen—white card, ground glass, pavement, or wall—whether the projection be through a telescope, or the 'pinholes' made in a cardboard, or between the leaves of a tree. (Projection, on ground glass or white cardboard, through a telescope, is of course the best way for timing the four contacts of sun and moon.) The little 'decrescent' or 'crescent' suns upon a wall or pavement are very pretty to watch and can easily be photographed. Nor is it the image only of the sun itself that becomes bow-shaped, but also the shadows cast by it. I first noticed this in the very large partial eclipse of 1912, seen in Paris, as an eclipse something between total and annular, when the sun was high in the heavens at noon, and standing on the flat roof of a house in Greenwich, I looked down on my shadow far below on the pavement. This showed my ears pointed like the traditional faun's; and, more curious still, when I stretched out my open hand, all my finger-tips were drawn out and bent like a bird's claw.

By the time that the sun has become a narrow bow in outline, and there remain but a few minutes, perhaps three or four, before the actual beginning of totality, the shadow-bands should begin to dance in streaks or wavy lines. I have heard these compared to many things, but I have never had the good fortune to see them myself; to the shadows of the ripples on the surface of the clear water dancing on the shingly beach below; to the shadows cast on the wall of a room from rippling sunlit waters outside. In the words of Mr. C. L. Brook (at the 1900 eclipse), "ripples raised on water by a bright breeze represent best what may be termed the structure of the shadows; they all move in the same direction, each ripple element is linear in character but retains its individuality only for a moment, appears to dissolve away and others take its place." In the Australian eclipse of 1922, some observers saw a flickering shadow hovering *over* distant trees eleven minutes before totality; later, on the ground, round their feet: "they flitted under the trees as the shadow of a bird might when flying above." Immediately after totality, the whole light was crimson for about ten seconds, and *red* and *black* shadow-bands raced over the ground. The bands were seen by chromospheric, not by sunlight. From the scientific point of view, the ideal occasion for their observation was in the American eclipse of January 1925, when Nature itself provided a white carpet for them to dance upon. It has been thought that they were due to currents in our upper atmosphere and that we could learn from them something about this; but the Americans observed them too closely and found that they were wind swirls—little 'devils' they are called in India—close to the ground, and in one place were seen to be chasing one another in all directions round a tree.

Whether the onrush of the moon's shadow can be seen at the coming eclipse will depend on whether the weather is clear or hazy. At the American eclipse it was looked for, but was undefined because of the haze. Major Hepburn says that this is the most impressive feature of the spectacle: "the onrush from a certain direction of rapidly spreading and engulfing darkness." But those who have work to do during totality have to forgo this experience.

The eclipse of June 29 is a young one; it is total—and not very much total at that—for the first time; when it is a Saros older it will be better. As a consequence, the chromospheric ring will probably be visible throughout the whole period that the sun's disc is covered. The form and size of the corona will depend on the actual state of activity of the sun at the moment. If there are many and great sunspots and prominences, and if these are near the sun's edge, then we may expect a complex

inner corona, which, however, will only show to the naked eye as a bright silver ring surrounding the narrow pink circlet of the chromosphere, with long coronal streamers extending in all directions; if there are no spots and insignificant prominences on the sun, then the coronal wings will lie folded along the sun's equator. Prominences, of course, will be visible to the naked eye only if large enough; keen sight could detect a prominence of 40,000 miles height; one of 200,000 miles should be easily visible. The corona will be ivory or white in colour; the chromosphere will be rosy; prominences will probably be red, but they may be white.

We would wish for June 29, that the weather on the sun should be stormy with many eruptions; that—for naked-eye observers—there should be deep lunar valleys near the points of second and third contacts; and that great red prominences should show up where these lunar valleys are.

In the American eclipse of 1925, New York City had a very beautiful aspect of the eclipse, which there was of short totality. The eclipsed sun showed as a faint circle of light with an intensely brilliant place at one spot, the rays from which spread out and gave the impression of a ring with a big diamond inserted in the usual way. There was a depression in the moon's edge which grazed the edge of the sun for some time, as seen from New York, which lay on the edge of the shadow track, just as Liverpool lies (probably) on the edge of the shadow track on June 29 next. In one of the Russian chronicles there is a description of the eclipse of May 1, 1185: "In the evening there was an eclipse of the sun. It was getting very gloomy and stars were seen, and in the men's eyes was a green light. The sun became similar in appearance to the moon, and from its horns came out somewhat like live embers"; which seems to point to something like the 'diamond ring,' perhaps a prominence as well as sunlight.

I have myself only once seen the eclipsed sun with the naked eye, and then it seemed small, much smaller than the full sun and moon to which I have been accustomed. But this was in India, with the sun high in the heavens. Mr. H. B. Adames saw the beginning of the eclipse of 1905 as the sun rose over Lake Winnipeg, and to him the entire body of the moon, which suddenly appeared with a blood-red prominence on either side, was a "huge black ball immensely exaggerated."

The effect of the rapidly diminishing sunlight on land and sky is weird and uncanny; all life and warmth fade out of the landscape; the flowers look withered, the grass and trees lose all trace of their living green and become of a dull lead colour; even the very faces of the observers become livid. If the sky is blue, it changes to a deep funereal purple. If there are small clouds near the sun, the iridescent colours, which are always there but not always easily seen, stand out vividly as in a spectrum. Round the horizon—which is near or beyond the shadow's edge—there is a glow of an angry gold, a sulphur light, not untinged with red. These are sunset effects, perhaps, which become visible when the daylight is cut off, but they may owe something also, as Mr. C. T. Whitmell suggested, to the red chromospheric light. If clear, it will be well to glance at the shadows cast by the corona; in 1922, Miss Miriam Chisholm found that these, though present, were undefined.

In my opinion, all naked-eye observers who are artists should make the painting of the colour effects on land, sky, and sea their chief study at this eclipse. It is not, of course, possible to do this work actually in the short totality, but the outline should be drawn before the eclipse, the colours noted during the total phase, and the painting finished afterwards while the tints are still fresh in the memory. Like meteorologists, artists get their results, whether the eclipse is clear or cloudy.

## Eclipse Photography.

By C. P. Butler.

ALTHOUGH the total solar eclipse on Wednesday, June 29, will be one of short duration—about 23 seconds total obscuration in England, and 30 to 40 seconds in Norway—the improvements in sensitivity of photographic materials and design of optical equipment warrant the attempt to obtain photographic records of various stages of the phenomenon. In order to give information wanted by observers having diversity of equipment, we may briefly summarise the various means with which observations may usefully be undertaken.

*Ordinary Cameras.*—These may range from the modest hand-camera to the largest field or studio apparatus, preferably fitted with some controllable exposing apparatus other than merely the lens cap. These may all give interesting pictures, but the scale of the result will be strictly proportional to the focal length of the lens employed. The diameters of the sun and moon are about half a degree of arc on the sky, so it is a simple matter to calculate what the actual linear diameter of the image of the eclipsed sun will be with any given

lens; an ordinary camera of, say, 7 inches focus, will give an image about $\frac{1}{160}$ inch diameter; one of 12 inches focus an image about $\frac{1}{100}$ inch diameter. In spite of the small scale, such lenses in previous years have given very beautifully defined pictures of the eclipsed sun and the surrounding corona. For the delicate outer streamers of the corona such apparatus is very efficient, and possibly more suitable than the larger scale instruments. This was illustrated by the success of Mrs. Maunder at the eclipse of 1898 in India, when the photographed coronal streamers extended about eight solar diameters from the edge of the moon. In this work of photographing an illuminated *area*, the power of the lens is regulated by the ordinary *f* value, *i.e.* the ratio of aperture to focal length, so that lenses with the largest *a*/*f* value available should be chosen for this occasion.

*Telephoto Cameras.*—By means of an auxiliary lens, generally concave, to reduce the bulk of the apparatus, increased focal lengths may be obtained with a given camera. Photographers having telephoto attachments to their lenses will be able to obtain large-scale pictures, but if the equivalent focal length is more than about 20 inches, some method of moving the camera with the sun will be necessary for exposures longer than a few seconds.

*Long-focus Lenses or Telescopes.*—These instruments are usually of larger aperture than a telephoto lens of similar focal length would be, so that, even when stationary, a useful picture may be possible with a snap-shot or short-time exposure. If furnished with clock-work drive, they provide perhaps the most important means of obtaining details of the structure of the eclipsed sun. Good results may be obtained by adjusting an ordinary camera, with lens set for infinity, immediately behind the eyepiece of a visual telescope which has also been adjusted on the distant object. If this arrangement is selected, it would be best mounted on a long, stiff board, and the whole fixed on a firm support pointing to the sun. The exposure should be made by rubber tube or antinous release, to avoid vibration.

*Stationary or Clock-driven Apparatus.*—With such a short eclipse it is doubtful if anything will be gained by installing temporary driving apparatus to follow the movement of the sun. Unless the utmost rigidity is secured, the small errors in fixing, driving, etc., may be greater than the actual movement of the object. The best advice appears to be that the camera should be firmly fixed on some rigid support, provided with a wedge adjusted to the elevation of the sun (about 11° in England). With instruments of moderate focal length the movement during the possible exposures will be scarcely perceptible. The movement may begin to be effective when focal lengths of more than 20 inches are employed.

*Alteration of Eclipse Track.*—Notice should be taken of the recent alteration of the track of the eclipse and its time of occurrence. The latest calculations, kindly furnished by Dr. Comrie, of the "Nautical Almanac" Office (*Monthly Notices, Royal Astronomical Society*, vol. 87, April, p. 496), place the central line about *one mile farther north* than the previously published track, so that observers located near the southern limit as shown on the Ordnance Survey Map should move to some station at least two or three miles northwards, to be certain of complete obscuration. Observers near the northern limit of totality will be better situated in consequence of the alteration. The time of occurrence is only about 4 seconds later than the published time.

Perhaps a word here as to the actual method of observing the instants of totality will be welcome to those who have not previously seen a total eclipse of the sun. The moon commences to enter on the sun's north-western edge, about the position of 2-hours on a clock face as one faces the sun, and will gradually cross the disc in a slanting direction, roughly towards the 8-hour position. When near totality, about an hour after first contact, the sun will be 25° north of the east point of the horizon and at an elevation of about 11° above the horizon, these figures being slightly different at various localities. As the advancing edge of the moon gradually approaches the eastern edge of the sun, the intense white arc of sunlight will be broken into small beads, produced by the rough mountainous edge of the moon. The instant when the last of these bright spots disappears is the time of second contact, and denotes the beginning of totality. Unless the observer wishes to obtain a series of records, the exposure should not begin until this instant of second contact, otherwise the delicate features of chromosphere and corona will be lost in the bright glare.

It is advisable to protect the eyes with dark glasses, such as may be made from a densely fogged photographic plate, or ordinary deep-tinted neutral glass, until totality occurs. From second contact to third contact is the period of totality, which in England is expected to last for about 23 seconds. This is the time available for securing pictures of the corona, which can never be seen except in these special circumstances, and the exposures to

be given will depend on the number of pictures to be taken. In general it would be advisable to limit the number to three:

(i.) A snap-shot just after the last beads of sunlight are seen to disappear at the moon's eastern or advancing edge. This will give the prominences and perhaps the inner corona.

(ii.) A longer exposure of, say, 15 to 20 seconds, depending on the time occupied in changing plates.

(iii.) Another snap-shot just as the bright beads appear at the western edge of the black moon.

*Cinema Record.*—The eclipse, being of short duration, may be specially suitable for the taking of a cinematograph film of the various stages, in which case the exposures would be started some time before totality, to show the transition from partial to total phase.

Intending observers who are able to go farther afield will find the photographic conditions somewhat more promising in Norway, where the altitude of the sun during totality will be about 21°.

*Photographic Plates.*—In general it will be best for the photographer to use the plates to which he is accustomed. All the well-known brands of material, glass plates and films, are now so uniformly good with regard to clean working that any choice may be governed by whatever special features it is desired to obtain. If only one exposure is to be attempted, the most rapid emulsion is to be recommended. All the chief makers list plates from 350 to 650 H and D. For the corona much of the light is concentrated in a few special colours, violet, blue, green and red, the combination giving the silvery white sheen characteristic of the solar envelope. Ordinary plates, of whatever speed, will record only the violet and blue portions. To get the advantage of the visible colour action, panchromatic plates may be used. The outstanding plates of this variety are the Ilford Iso Zenith, Iso Wellington, and Imperial Eclipse Ortho, all being of the highest sensitivity and very clean working. Most makers also supply Spectrum Panchromatic Plates, which are of somewhat lower speed, but give a more equable representation of the spectrum colours. When the apparatus is fitted with lenses of large relative aperture, these plates may be used instead of those of greatest rapidity. The easiest way of treating these colour-sensitive plates is to develop in darkness for six minutes with the metol-hydroquinone solution recommended by the makers. For those, however, who cannot resist the temptation to enjoy seeing the image gradually built up, use may be made of one of the various desensitising solutions which are now available. Before development, and in darkness, bathe the exposed plate in a weak solution of the desensitiser, such as phenosafranine, for about one minute. Then add the developer, and continue for a further six minutes, during which an ordinary safelight may be used to see the progress of development.

There is one possibility of difficulty, owing to the early hour (6 h. 23 m. A.M. British summer time) at which the eclipse will take place. If the sky is slightly hazy, the contrast of the silvery-white coronal structure may be lessened with respect to the sky background. With ordinary plates this cannot easily be remedied. But if panchromatic plates are employed, much of the sky glare may be eliminated by using a moderately tinted colour screen, of the type known as $K_3$, which will cut out much of the blue glare, and yet pass sufficient of the green, yellow, and red to the panchromatic plate to give good density in the image.

*Supersensitised Plates.*—For observers with more experience, most varieties of plates may be hypersensitised by a preliminary ammonia bath. The plates are simply bathed in a weak solution of ammonium hydrate (about 1 per cent.) for one minute, at a temperature of about 50° F. (14·5° C.). Then dry the plate as quickly as possible, either by a centrifuge or a fan. All this should be done in darkness. If an alcoholic solution is preferred, the plate will dry much quicker, but the resulting sensitivity will be slightly lessened.

*Films.*—Observers desiring to use films may do so with every confidence; and as many film emulsions are now made colour-sensitive, the remarks already made as to using colour screens, if glare is present, also apply to films.

*Colour Photography.*—Many observers intimate their intention to try for colour records of the eclipse phenomena, for which several processes are available.

*Backing.*—It is advisable that all plates should be backed, as any traces of halation due to bright portions of corona, prominences, or slight cloud may otherwise diminish the delicacy of detail.

*Shadow Bands.*—In addition to the corona and prominences, there are several other features peculiar to a total solar eclipse which may be of interest to photographers wishful of a more extensive programme. Just before totality, and immediately after, a series of moving wavy shadows are usually seen on the ground or sides of buildings facing the eclipsed sun. The cause of these is not yet known with certainty, but they appear to have some relation to prevailing atmospheric conditions. If a white sheet or a whitewashed wall can be used,

photographs of these shadow-bands will be of value. Exact details of the position of the flat surface, whether horizontal or vertical, its direction with regard to north, south, etc., should be carefully noted. Also the actual width, rate of motion, and general direction of the bands on the white surface, and which way they move.

*The Moon's Shadow.*—This applies chiefly to observers who may be fortunate in occupying a hill station in Wales, Lancashire, or Yorkshire. The dense black shadow may be seen travelling over the landscape, and would form an interesting photographic study wherever possible. Only snap-shots may be made on this, owing to the great velocity of the moon's shadow over the earth's surface.

*Landscape Colours.*—Very varied descriptions of the remarkable changes of colour of landscape features have been noted during previous eclipses. Those provided with panchromatic plates or colour plates may do useful work in recording any of these changes. In such cases photographs of the same landscape under ordinary daylight conditions should be obtained to indicate changes due to the eclipse.

*Spectroscopic Photographs.*—So far we have only considered the photography of the eclipsed sun as a picture. There must be many observers who could photograph the spectrum of the phenomenon, and quite valuable records may be made with simple equipment. Either a prism or diffraction grating may be used, adjusted in front of the camera lens. In general this will involve some deviation of the pointing of the camera, which will have to be directed away from the line of the sun by an angle equal to the deviation produced by the prism, etc. Prisms and gratings can, however, be obtained which are arranged to give direct vision, and in such cases the procedure is exactly the same as with the camera lens alone. Very good results can be obtained with the replica gratings produced by the Thorp process, an additional feature being their relatively small cost compared with that of an original ruling on speculum metal. The photographic manipulation is identical with that for ordinary pictures, except that it is scarcely worth using any except panchromatic plates, which will record the whole of the visible spectrum.

*Time Records of Exposure.*—The scientific value of any records obtained will be greatly enhanced if the instants of exposure are noted as accurately as possible. The track of the eclipse passes through such populated areas that few observers will be far away from some place where it should be possible to get facilities for listening to the standard time signals which will be broadcast from Greenwich and Paris. The well-known Greenwich six dots, the sixth indicating the time to be noted, will be broadcast at 6 h., 6 h. 15 m. and 6 h. 30 m. A.M. British summer time on June 29. (The latest arrangements for the broadcast time-signals will be found in "Our Astronomical Column.") Perhaps it would be a general convenience if the present 1.0 P.M. signals from Big Ben (12.0 G.M. noon) were given as Greenwich dots for the week preceding the eclipse. The Paris Observatory is also broadcasting from the Eiffel Tower (FL) rhythmic and other time signals specially for the eclipse on both spark and continuous wave.

Any records which can be made to the nearest second will be comparable in accuracy with the determinations of actual beginning and ending of the eclipse itself.

To summarise briefly :

(1) Arrange for a steady support for the camera directed to the sun, special care being taken if there should be a high wind.

(2) Provide the camera with a lens of as large a relative aperture as possible.

(3) Use rapid plates, preferably panchromatic, with a light-yellow screen, and have the plates backed.

(4) Exposures should be from snap-shot to about 10, 15, or 20 seconds for cameras up to 20 inches focal length. For longer cameras it is better to confine exposures to about 5 seconds, except in the case of clock-driven instruments.

(5) For clock-driven apparatus take three exposures: snap for prominences at beginning, 15 to 20 seconds during totality for corona, and snap for prominences at end of total phase.

## Radio Telegraphy and the Eclipse of the Sun.

SINCE 1912 observations have been made from time to time on the effect of solar eclipses on wireless signals. Up to 1925, however, the data obtained from the observations made was untrustworthy and contradictory, the reason for this being that the observations depended on estimates by the ear of the intensity of signal strength. Apart also from the absence of accurate and suitable measuring apparatus, it is only in recent years that the mechanism of the propagation of waves round the earth has been understood to a sufficient extent to indicate the type of observation likely to yield positive results. The path of totality of the eclipse which took place on Jan. 24, 1925, crossed the United States, and the opportunity was then taken of arranging for a

network of observing stations on a comparatively large scale. At several of these stations galvanometer-recording methods were employed. During the same eclipse, observations of the field strength and direction of arrival of wireless signals transmitted from America were made in Great Britain. By that date also the theoretical work of Eccles and Larmor had provided a hypothesis of wave propagation by which the results obtained could be tested.

To understand the effect a solar eclipse is likely to have on wireless signals it is necessary to review briefly the manner by which it is now believed that electromagnetic waves are propagated through space. From any transmitting aerial part of the energy emitted is propagated horizontally along the surface of the ground and part is propagated in an upward direction into the atmosphere. The resistance of the earth has an absorbing action on the waves travelling over its surface. This absorption is least with long waves, and becomes very great in the case of what are known as ultra-short waves, that is, waves below, say, 100 metres. Owing to various cosmic influences, particularly radiation from the sun, the earth's atmosphere is ionised. During the daytime ionisation occurs at comparatively low levels, but with sunset recombination takes place in portions of the atmosphere where the gas pressure is fairly large, and by night, only the upper atmosphere at a height above 70 kilometres is left in an ionised state.

The effect of the ionisation of the atmosphere on electromagnetic waves has been shown by Eccles and Larmor to be twofold. First the electrons or ions present are set into vibration by the waves. As the electrons move they set up small electromagnetic waves out of phase with the original wave. The combination of the electric forces of these two waves causes the upper portions of the waves to appear to travel with a greater velocity than the lower portion, so that the wireless waves in the ionised medium are bent towards the earth. The amount of bending produced has been shown to depend on the intensity of ionisation and upon the square of the wave-length. Larmor has shown, however, that another effect of the ionisation present must be taken into account. This factor is the loss of energy due to collisions between electrons and gas molecules. This loss produces an absorbing effect on the waves passing through the medium, and in order that the effect may be small, the electrons must have a long mean free path; therefore the bending effect on the waves can only take place in the upper regions of the atmosphere. It would be expected on this theory that the absorption due to the ionisation in the atmosphere would be less with very short waves of high frequency than with longer waves, since the time between a collision of an electron and a molecule will be greater than the frequency of the wave.

The general result is that upward radiation from an aerial may be pictured as struggling upwards through the absorbing lower atmosphere until it reaches a level at which it is bent round the earth without loss, again enters the absorbing layers, and finally reaches the earth once more. At night the absorption will be less, since the ionisation due to sunlight in the lower levels will be absent. Thus in general, wireless waves are propagated to greater ranges at night except in the case of extremely short waves, less than, say, 16 metres, the frequency of which is so great that the absorbing action is small, and a considerable intensity of ionisation is necessary to bend them round the earth. Such very short waves will therefore travel better by day than by night. It will thus be seen that, at short distances, a wireless receiver will be affected chiefly by waves which are directly transmitted along the earth's surface or through the lowest levels of the atmosphere; at intermediate distances the received signal will be the resultant of the direct ground wave and a wave travelling by the upper atmosphere; while at very long distances the effect on the aerial will be solely due to waves travelling through the upper layers of the atmosphere.

It follows from the above that the general effect of a solar eclipse should be similar to that of a sudden and very rapid sunset, followed by a sunrise. As the moon's shadow sweeps through the atmosphere, a recombination of the ions present will take place, with the result that the agencies producing the bending and absorption of the waves become confused and unsettled. A short time later, however, the lower atmosphere becomes clear of ions and night conditions prevail until the shadow passes on. The effect on the intensity of wireless signals during the eclipse should therefore be first a decrease in the average intensity followed by an increase. Broadly speaking, this was the result generally obtained in the 1925 experiments. The relative and absolute amplitudes of the drop and rise in signal strength were found to depend, however, on the position of the transmitting and receiving stations to the path of the shadow and on the wave-length used in the observations.

It appeared to the Radio Research Board of the Department of Scientific and Industrial Research that it was desirable that every endeavour should be made to use the opportunity offered by the eclipse on June 29 next for carrying out further observations on its effect on radio telegraphy. After discussion with other interests involved, a programme of observations has been drawn up which it is hoped will yield useful results. A point brought to the notice of the Board by Dr. E. H. Rayner, of the National Physical Laboratory, which has not previously been taken into account in eclipse observations, is that, as the altitude of the sun is low, the line on the earth's surface vertically over which that portion of the ionised layer lies in which the bending of the wireless waves probably takes place, is about 100 miles to the south-east of the line of totality on ground level. In the experiments to be carried out this fact is being borne in mind. In one set of experiments which are to be carried out under the supervision of Prof. E. V. Appleton, observations will be made by photographic recording apparatus on wireless signals crossing the line of ground totality—transmitted from Newcastle and received at Liverpool. Similar simultaneous observations will be made on transmissions from a station to the south of the ground totality line and received at the Radio Research Station at Peterborough.

The object of the experiments is in particular to investigate the height at which the bending of the waves by the ionised layer takes place during the eclipse and to endeavour to note the changes in the height of this layer. The method to be used is that described by Prof. Appleton in his paper in the *Proceedings of the Royal Society* (vol. 109, A, p. 621, 1925), whereby the wave-length of the transmitting station is varied through a small known amount (for example, 5-10 metres) in a given time (for example, 10-30 seconds), and the height of the layer is calculated by counting the number of interference fringes produced by the interference of the ground and atmospheric rays. The number of fringes produced depends on the path difference between the rays. From this number a simple calculation gives the height at which the bending of the rays takes place. It has been possible to arrange for the necessary transmissions for these experiments by co-operation with the British Broadcasting Corporation.

In connexion with previous work of the Radio Research Board, long-wave signal-strength measuring apparatus has been developed by Mr. J. Hollingworth and installed at the Radio Research Station at Slough, at the University of Aberdeen, and at University College, Exeter. This apparatus will be employed for accurate signal-strength measurements on long-wave stations during the eclipse. The distance at which the ionisation of the atmosphere comes into play is greater in the case of long waves than with short waves. The necessary transmissions for observations on long waves are, accordingly, being arranged from continental stations under the auspices of the Union Radio-Télégraphique Scientifique Internationale. Long-wave intensity observations are also to be carried out with the apparatus developed by Prof. E. W. Marchant at the University of Liverpool.

Recent research has shown that the variable errors in radio direction-finding are due to the interaction of the magnetic fields of the ground and atmospheric waves. Under certain conditions these two forces give a resultant magnetic field not at right angles to the direction of the transmitter, and therefore an error in radio direction-finding apparatus is produced. The direction of the magnetic field of the atmospheric wave depends, it is believed, on the ionisation of the upper atmosphere and on the relation of the direction of transmission to the magnetic field of the earth. To test this explanation of directional errors, observations are to be carried out by a network of direction-finding stations observing transmissions from the London and Manchester broadcasting stations. Finally, accurate records of the intensity of atmospherics on a selected wave-length and of the direction of arrival of individual atmospherics are to be made at the Radio Research Station, Slough, and in Scotland.

In addition to the above experiments, which are being carried out directly under the supervision of the Radio Research Board, the Radio Society of Great Britain is arranging for observations by its members on transmissions on 90 metres from a station at Caterham and on transmissions on 100 metres from a station in Iceland. Arrangements are also being made for transmissions on 23 metres and on a wave-length between 44 and 46 metres. These two latter stations will be situated one to the north and one to the south of the line of totality on the ionised layer.

A difficulty which presents itself in connexion with the present eclipse is that totality takes place so early in the morning that ordinary sunrise effects may not have entirely ceased and may mask to some extent the eclipse effects proper. In order to prevent the confusion of any eclipse effects with those due to other causes, all the

observations proposed will be carried out on the mornings of the two days preceding the eclipse and on the two days following, in addition to the actual morning of the eclipse. The transmissions on which observations are made will extend for a period of two to four hours on each of these days.

How far the results of the experiments proposed will be valuable in checking present hypotheses of the propagation of wireless waves or in providing new information on this subject cannot, of course, be definitely stated beforehand.

O. F. B.

## Astrophysical Eclipse Problems.

By Prof. H. Dingle.

If direct observation of the sun were our only means of investigating the form of our luminary, we should learn little more than that it is an incandescent rotating sphere. The spectroscope makes possible the beginnings of a structural analysis by revealing an absorption spectrum —that is, a continuous spectrum crossed by relatively dark lines. This assures us that, broadly speaking, the sun consists of two portions—an interior responsible for the continuous spectrum, and an atmosphere the constituents of which selectively absorb some of the light of the interior, each according to its kind. Whatever can be learnt from the continuous spectrum characterises the interior, while the study of the dark lines—the Fraunhofer lines—is the study of the atmosphere. In the ordinary solar spectrum, of course, both continuous and dark line spectra appear together. Neither can be obtained, in the first instance, apart from the other, so that the separate analysis of 'interior' and 'atmosphere' is greatly complicated. Some measure of success has nevertheless been achieved; for example, the effective temperature corresponding to the light from the interior has been determined from the continuous spectrum, while the spectroheliograph affords some knowledge of the distribution of a few types of atom in the solar atmosphere. But on the whole it may be said that the inevitable association, in the ordinary solar spectrum, of the atmospheric lines and the photospheric background is a great hindrance to the complete study of the respective regions of the sun—and particularly that of the atmosphere.

Observations of the sun's limb, where the atmosphere might be expected to appear alone and to show a bright line spectrum, reveal only that it is too thin (in angular measure) to do so. Even the spectroscopic method devised independently by Lockyer and Janssen in 1868 shows only the higher reaches of the atmosphere at the limb, and it was not until 1896 that it was finally established that there exists a bright line limb spectrum at all comparable with the Fraunhofer lines. Since that time the long-focus instruments at the Mount Wilson Observatory have given images of the sun with sufficient depth of atmosphere for the bright line spectrum to be observed, but when produced in this way the lines are still encumbered by the Fraunhofer spectrum, and the facilities afforded for detailed study of the solar atmosphere leave much to be desired. Only during a solar eclipse, when the photosphere is at least partly obscured by the moon, can a pure atmospheric spectrum be obtained, so that for our knowledge of the structure of the sun's atmosphere—the solar meteorology, as it may be called—we still rely mainly on eclipse observations.

Three aspects of the bright line, or 'flash' spectrum, are studied in modern eclipse research; namely, the wave-lengths of the lines, their relative intensities, and the distribution in the solar atmosphere of the atoms or ions responsible for them. It appears probable that the determination of wave-lengths will in future be attempted only by the method described below (p. 91) by Prof. Fowler, for which totality is not necessary. It need not, therefore, be referred to here except to point out its uses in the identification of the lines and the recognition of disturbing influences in the event of displacements being established with respect to terrestrial standards or the Fraunhofer spectrum. Two facts must be borne in mind in this connexion. First, the Fraunhofer spectrum is observed more or less radially, and the flash spectrum tangentially, so that there may be differences in the thickness and state of motion of the regions of the atmosphere observed; secondly, the lowest layers of the atmosphere can be observed only through an envelope of the higher layers, and any difference of wave-length at different levels will broaden the lines and possibly give rise to unsymmetrical reversals when the lines are examined microphotometrically.

The question of the relative intensities of the lines has greatly increased in importance with the development of modern spectroscopic theory. It is intimately bound up with the question of the distribution of atoms and ions in the sun's atmosphere; for the intensity of a spectrum line depends

both on the number of atoms of the proper kind which are present, and on the physical conditions tending to make them emit that particular line. It is necessary, therefore, not only to compare the relative intensities of different lines, but to investigate also the variation in intensity of each line in different regions of the atmosphere. Since it is only the atmosphere at the limb which is being studied, this is equivalent to a determination of the variation of intensity along each line, supposing a radial or tangential slit to be employed.

This investigation is attended by considerable difficulties. Not only is there the complication already referred to (namely, that the ends of the lines representing the lowest levels may contain light from the higher levels also), but also the diffused light in the earth's atmosphere tends to fill the slit and make the distribution of light in the spectrum line differ from that in the corresponding regions of the sun's atmosphere. This effect is very pronounced in misty weather, and on such occasions, when a radial slit has been used, bright lines have been observed to extend even over the whole diameter of the dark moon. Objective prism spectrograms (*i.e.* spectrograms obtained by placing a prism before the object glass of an astronomical telescope, and using neither slit nor collimator) in which each 'line' is a crescent image of the region of the solar atmosphere emitting the corresponding wave-length, do not suffer from this defect, for the diffused light, entering the telescope from all directions, is simply spread over the photographic plate to produce an imperceptible fogging. In these spectra the lengths of the arcs measure the heights in the sun's atmosphere reached by the corresponding emitting sources, but irregularities in the moon's limb and other interfering agencies invest the results with a considerable amount of indefiniteness. All photographic methods of attacking this problem have to cope with the further difficulty of non-uniform sensitivity of the plates to light of different wave-lengths; thus, two lines of the same length in different parts of the spectrum do not necessarily arise from emitting sources similarly distributed in the sun's atmosphere. Results obtained from many eclipses will have to be carefully analysed and compared before trustworthy conclusions in any degree of completeness can be reached.

The beautiful photographs obtained by Merfield at the eclipse of January 1926 suggest that the method employed by him (it was in part proposed also by Lockyer in 1896, but clouds prevented the trial) might yield valuable results. It consists in photographing an objective prism flash spectrum on a plate moving uniformly at right angles to the direction of dispersion, immediately in front of the plate being a narrow slit, lying along the spectrum, so as to reduce the instantaneous image of each 'arc' practically to a point. The resulting spectrum then appears as a set of parallel lines of various lengths, and it is proposed to determine the heights reached by the corresponding emitting sources from a measurement of the rate of change of intensity along the lines. It remains to be seen what degree of success the method is capable of yielding when the actual determinations have been made.

When the general distribution of the various types of atom and ion in the solar atmosphere has been reliably determined, the application of recent spectrum theory will give much information concerning the physical conditions existing there. A particular investigation of this character is being undertaken on June 29, in which attempts will be made to determine the relative intensities in the chromosphere of the $Ca^+$ diffuse 'doublet' at λλ8498-8662 in the infra-red, and the bright counterparts of the well-known H and K Fraunhofer lines in the violet. The result should have considerable significance in connexion with the theory of the calcium chromosphere proposed by Milne. Photographs extending still further into the infra-red, up to about λ15,000, are also to be attempted.

These eclipse researches into the constitution of the sun's atmosphere are of great interest and importance, but they must always play second fiddle to the study of the corona, because it is only during a total eclipse of the sun that the corona can be observed at all. The investigations referred to above are greatly facilitated by the intrusion of the moon, but they could be carried out in some manner if the moon did not exist. (It must not be forgotten, however, how much the methods employed on the uneclipsed sun owe to knowledge gained originally from eclipse observations.) But if there were no moon, or if the moon were slightly farther from the earth than it is, we should not to this day suspect even the existence of the corona. It is obvious, therefore, that whatever else may be neglected at a total eclipse, all possible information must be obtained with regard to the corona.

Our absolute knowledge of the corona may be summed up very briefly. It is an intricate solar envelope the form of which varies with the phase of

the sunspot period, while its light, which is partly polarised, consists of a combination, in varying proportions, of unknown bright lines, continuous spectrum, and Fraunhofer spectrum. Considering that the corona is observable, on the average, for at most about three minutes every two years or so, it is not surprising that our knowledge of it is so rudimentary. Single observations have no definitive value; they need confirmation at subsequent eclipses. In the case of the corona they have perhaps more often been contradicted, so that uncertainty exists whether the observations are at fault or whether the corona has changed. In these circumstances progress is necessarily slow, and many records must be accumulated before conviction can be reached.

A record, as complete as possible, of the form of the corona at each observable eclipse has been kept for some years, and must of course be continued indefinitely. In time this should reveal any obvious periodic changes of form or structure which may exist in addition to the already known relation with the sunspot period. The connexion between the corona and the solar prominences might also be elucidated from such a record. The difficulties, however, are enormous, for not only are there long gaps between successive photographs or drawings, but also at each eclipse the coronal light seen is at each point an unanalysable integration of light emitted along a chord of the coronal shell, and does not represent a simple plane section of the shell. The changes in the form of the corona appear to be slow. Attempts have been made to detect them by observing the same eclipse near sunrise and sunset, but the results are inconclusive. Further attempts are desirable. For such observations to be successful there must be convenient sites near the two ends of the belt of totality and the sun must be unclouded at both stations—a combination of circumstances which very rarely occurs.

The total light of the corona still awaits a satisfactory determination. It no doubt varies from one eclipse to another, and might be correlated with the sunspot period. Still more important, perhaps, is the law of variation of light with distance from the sun's limb. Several widely differing formulæ have been proposed, and here again there are probably changes from one eclipse to another. The heat radiation of the corona was measured by Abbot in 1908, but further determinations would be of much value. The very delicate apparatus designed by Callendar for use in the 1905 eclipse, which was spoilt by clouds, might give valuable results in this direction of research.

The spectrum of the corona and its connexion with the sunspot period are as yet very imperfectly understood. The portion of the light which gives a Fraunhofer spectrum comes from the middle and outer corona, and is undoubtedly reflected or scattered sunlight. Experiments on the amount and kind of polarisation of this light at different distances from the sun's limb are necessary to afford an insight into the mechanism of the reflection or scattering, and hence into the physical constitution of the corona. Such experiments have been made at many previous eclipses, but the difficulties of interpreting the results are so great that further investigations by the most trustworthy methods are necessary. The continuous spectrum of the inner corona may also be due to photospheric light, but there are reasons for thinking that it originates in the corona itself. Confirmation is difficult, but the evidence most obviously necessary is that of the curve of distribution of energy throughout the spectrum.

The line spectrum of the corona offers perhaps the largest field for investigation. None of its lines has yet been recognised in the laboratory, and its composition is very uncertain. Much progress has been made in disentangling it from the chromospheric spectrum, but finality has not yet been reached, and the faintness of the spectrum makes it almost certain that the present lists of lines are far from complete. Variations in the relative intensities of the lines at different eclipses suggest that it is a superposition of two or more spectra. Several attempts, on numerical as well as observational bases, have been made to classify the lines into groups, but the results so far are not consistent. The spectrophotometric measures initiated by the British expedition to Sumatra in January 1926 indicate a promising method of classification in terms of the variation of intensity along the lines. When such data have been obtained at a number of eclipses, accidental similarities can be eliminated and possibly definite conclusions arrived at.

The precise wave-lengths of the coronal lines are important for both laboratory identification and measurement of motions in the line of sight. There is so little agreement as yet between the various sets of measures available that further determinations with the highest possible degree of accuracy are of the greatest importance. It may be hoped that Prof. Fowler's experience this month

with high-dispersion photographs of the 'flash' spectrum will show that the same method is practicable with the corona—at least so far as the brighter lines are concerned. The line spectrum of the corona appears to be relatively brighter at maximum than at minimum of the sunspot period, so that the coming eclipse should be favourable for the various investigations connected with it.

It will be realised that the outstanding astrophysical eclipse problems are 'many and various.' We may reasonably hope that, granted fair weather, our English eclipse will lead to important extensions of our knowledge of the sun.

## Spectroscopic Observations during a Partial Eclipse of the Sun.

By Prof. A. Fowler, F.R.S.

IT is common knowledge that the chromosphere and prominences which surround the visible surface of the sun cannot be seen in the telescope at ordinary times because they are less bright than the diffused light of the sky on which they are superposed. They can, however, be observed by combining the telescope and spectroscope in the manner discovered by Lockyer and Janssen in 1868. The spectroscope being adjusted on the bright red line of hydrogen (H$\alpha$), and an image of the sun being focussed tangentially to the slit, the diffused sky light is spread out into a continuous spectrum (crossed by dark lines) and is thereby so much reduced in intensity that the bright hydrogen line from the chromosphere, or from a prominence, becomes easily visible. To see the actual forms of the chromosphere and prominences, it is only necessary to open the slit rather wide.

Other bright lines besides H$\alpha$, including the yellow line of helium, $D_3$, and the hydrogen line H$\beta$, may be observed in the same manner, but they are not numerous when instruments of moderate size are employed. Spectroscopic observations with large telescopes at ordinary times, or with ordinary instruments during total eclipses, however, have shown that as the sun's edge is approached the bright line spectrum increases in complexity and finally exhibits a multitude of bright lines which originate in a region extending less than two seconds of arc above the photosphere, the apparent diameter of the sun being nearly two thousand seconds of arc.

When observations are made near the central line during a total eclipse, the spectrum of this shallow layer suddenly bursts into view at the beginning of totality, and almost as quickly disappears; it reappears for two or three seconds just before the end of totality, at the point of contact of the sun and moon. On account of its brief duration under these conditions, the spectrum of this shallow layer which surrounds the sun has been called the 'flash spectrum,' and the layer itself the 'flash stratum.' It is here that a large proportion of the absorption which produces the dark Fraunhofer lines of the ordinary solar spectrum takes place, and the flash stratum is accordingly also known as the 'reversing layer' of the sun.

Overlying the reversing layer, but not sharply divided from it, to a total height of about ten seconds of arc above the photosphere, is the chromosphere. This has not a smooth continuous surface,

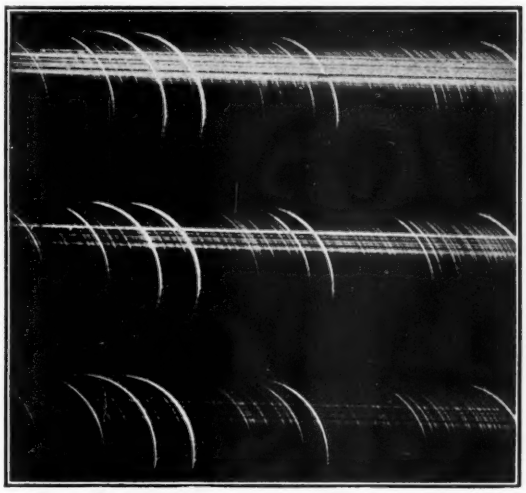


FIG. 1.—Flash spectra: portion of the first large plate taken with a 6-in. prismatic camera in India, 1898. By Prof. A. Fowler.

but is roughly serrated, and the prominences, which are sometimes very brilliant and reach to enormous heights, rise out of the chromosphere.

On ordinary occasions, with telescopes of moderate size, it is not possible to observe the spectrum of the reversing layer, because of the 'boiling' due to atmospheric tremors, which blends the bright lines from the thin stratum with the brighter spectrum of the edge of the sun's disc. Occasionally, however, there is a disturbance at some place near the sun's limb, and the reversing layer may then be so far elevated that a large number of bright lines can be differentiated from the photospheric spectrum. With the large instru-

ments available at the Mount Wilson Observatory, bright lines have been photographed in large numbers without an eclipse, but even here the flash spectrum is somewhat confused by that of superposed sunlight.

It is during total eclipses of the sun that the flash spectrum can be most effectively observed. The reversing layer is then revealed for a brief time on a comparatively dark background, with no interference from the bright disc of the sun. Its spectrum has frequently been successfully photographed by the use of slit spectrographs of ordinary type, and by the use of prismatic cameras or slitless spectroscopes. With the latter form of spectrograph a spectrum taken near the beginning of totality consists of a succession of curved images of the portions of the chromosphere and reversing layer visible at the moment of exposure, each representing a spectrum line, and having a length depending on the height of the gas or vapour which produces it. The nature of such photographs will be gathered from Fig. 1, which reproduces a small part of a plate containing ten such spectra taken at intervals of about one second, beginning a few seconds before the commencement of totality in India in 1898. In these photographs the reversing layer is represented by the numerous short arcs which appear in the middle of each spectrum, and the chromosphere by the longer arcs, among which those of calcium H and K, hydrogen and helium are very conspicuous. The bright streaks of continuous spectrum originate from specks of the sun's disc which were visible through irregularities in the edge of the moon.

Such photographs bring out many important features of the bright line spectra, but for various reasons they do not yield wave-lengths of a high degree of accuracy. Excellent photographs have also been secured with slit spectrographs, but in observations during totality, the dispersive power which can be utilised is limited by the short duration of the phenomena.

In order to obtain an increased duration of the flash spectrum, Evershed, in the eclipse of 1900, selected a station near the edge of the zone of totality. In these circumstances it will easily be understood that the dark moon must glide tangentially with respect to one point on the sun, so that the flash spectrum can be effectively observed near that point for a considerably longer time than from a place near the central line. Thus, using a prismatic camera, Evershed succeeded in obtaining good photographs of the flash spectrum during a period of about half a minute, and the illumination of the sky throughout this period was sufficiently reduced to allow of the fainter lines being registered on the plates.

Observations of the large partial eclipse which was visible in England on April 17, 1912, indicated another means of observing the flash spectrum for a comparatively long time, and suggested that photographs might be taken with instruments of large dispersion, comparable with those used for the sun itself. This possibility is opened up by the fact that at the cusps of the partially eclipsed sun the flash and chromospheric layers project to a greater distance than their radial depths. This will readily be understood from the diagrammatic representation in Fig. 2.

At South Kensington the magnitude of the

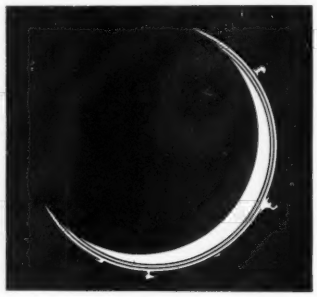

FIG. 2.

eclipse of 1912 at central phase was 0·92, and visual observations by Fowler showed that the flash spectrum could be effectively observed at the cusps during about thirty minutes. Some gain in visibility of the bright lines was first noted when the magnitude of the eclipse was 0·55, while the apparently complete flash spectrum was visible when the magnitude of the eclipse was 0·8 or greater (*Monthly Notices, R.A.S.*, **72**, 538). Similar observations were made at Cambridge by Prof. Newall (*Monthly Notices, R.A.S.*, **72**, 536).

In view of these observations, it seems possible to employ spectrographs of greater power than any hitherto used in eclipse work, and to impress comparison spectra for the accurate determination of wave-lengths. It may thus be hoped to obtain data for the investigation, among other things, of possible small displacements of the bright lines such as are already known in connexion with the Fraunhofer lines.

It was intended to make this experiment in Russia on Aug. 21, 1914, but owing to the outbreak of the War the expedition had to be abandoned, and the instruments were not returned to England until 1924. It is now planned to employ practically the same equipment during the eclipse of June 29 this year.

Although slightly better conditions might be obtained by the occupation of a station somewhat nearer the central line of eclipse, the instruments are being erected on the roof of the Imperial College of Science at South Kensington. The magnitude of the eclipse there at central phase will be 0·96, and the experience of 1912 indicates that this will be ample for the purpose in view. Any advantage likely to be gained by going farther north would, it is thought, not sufficiently compensate for the loss of the facilities afforded by the College laboratories and workshops. Prof. Sampson has also taken this view and will attempt similar work at the Royal Observatory, Edinburgh, where the greatest magnitude of the eclipse will be 0·98.

A large partial eclipse also provides a very favourable opportunity of investigating the spectrum of the sun near the limb. The observations at Mount Wilson have already shown that this spectrum differs very considerably from that given by the centre of the disc, but observations during a large partial eclipse may have the advantage that there will be no scattered light from the central parts of the disc superposed on the light emanating from near the sun's edge.

The spectrograph to be employed at South Kensington has a concave grating of 10 feet radius in an Eagle mounting, and will be adjusted for the second order spectrum so as to avoid undue astigmatism. An image of the sun about 2 inches in diameter will be formed in the plane of the slit by a 6-inch objective, which will receive light from a cœlostat after reflection from a second mirror. Adjustments are provided for maintaining the image of a cusp on the desired part of the slit, and it is expected that the exposures required will not be so long as to cover an undesirable range of solar latitude as the cusp changes its position on the sun. The requisite astronomical data for South Kensington have been specially computed by Dr. L. J. Comrie of the "Nautical Almanac" Office.

## The Forms of the Solar Corona and their Origin.

By Dr. William J. S. Lockyer.

It is only during total solar eclipses, when the moon comes exactly between the earth and the sun, and cuts off all the brilliant light of the disc, that an outer solar atmosphere of an exquisite pearly hue known as the 'corona' is revealed. Without such eclipses, this atmosphere, even with the aid of any of the great and ingenious optical means available to-day, would still be unknown. The corona is of very considerable extent, far exceeding, in proportion to the size of the solar disc, that of our own in relation to the size of the earth.

It is well known that the form of the corona varies in shape and brilliancy very considerably. Sometimes the form is very irregular, the coronal matter being extensively distributed all round the solar disc, embracing both the solar poles and the equator. This form is termed 'polar,' 'irregular,' or 'maximum,' as coronal streamers are situated near the solar poles (Fig. 1).

On other occasions the polar regions are conspicuous by the complete absence of streamers, and in their place beautifully curved rifts or plumes are seen, the long streamers being restricted more to the equatorial regions. This type of corona is termed 'equatorial' or 'minimum,' and is sometimes referred to as of a 'wind-vane' form, as it resembles this object (Fig. 2).

Finally, there is a third and also very pronounced shape which is intermediate between the above two forms. This is termed the 'intermediate' type or 'square' corona. In this case the streamers are generally concentrated in mid-solar latitudes, leaving the poles and equator comparatively free from any large coronal extensions.

The use of the terms 'maximum' and 'minimum' with regard to the shape of the solar corona referred to the epochs of sunspot maximum and minimum, and it suggested a connexion with the periodic variation in the spotted area of the sun's surface. Until a few years ago, it was generally concluded that sunspots were therefore the origin of the coronal forms, and their waxing and waning was reflected in the changes of these forms.

Sunspots, however, do not appear at or anywhere near the solar poles; the highest latitude they ever attain is only 45°, and then they are only of very small area. On the other hand, large coronal streamers and prominent rays are sometimes situated in very high latitudes; in fact, at times they are very near or at the poles, and consequently quite outside the regions of spot activity.

Moreover, at the epochs of greatest spotted area, the mean latitude of spots is only about 18°; yet it is precisely at about those epochs that the coronal streamers appear at the poles, and the coronal forms are described as 'maximum' or 'polar.'

FIG. 1.—Solar corona, Aug. 30, 1905. Maximum type. Exposure 20 sec. By C. R. Davidson. By permission of the Astronomer Royal.

These facts suggested to me in 1903 that the prominences, which are not only very important factors in the mechanism of the solar atmosphere, but also can and do appear in all solar latitudes, might be directly responsible for these changes of form of the corona.

The crucial test resolved itself into demonstrating that the epochs of the occurrence of all those forms of the corona which exhibited streamers at or near the solar poles should be coincident with those epochs when prominences were known to be in very high latitudes. To do this it was necessary to study all the trustworthy data relating to the more modern records of solar activity.

It is well known that the area or number of the spots on the sun varies from year to year, and that about every eleven years or so this number reaches a maximum value. This cyclical change of spotted area is shown at the top of Fig. 3. Sunspots are not formed on all parts of the solar disc, but are strictly confined to a belt which lies approximately between latitudes 45° and 5° on both sides of the equator. Sunspots, therefore, never occur at or near the solar poles. The mean yearly positions of these belts are shown in the third series of curves illustrated in Fig. 3. The latitudes of sunspots are closely associated with sunspot frequency. When there are most spots their mean latitude is about 18°: when there are fewest spots their mean yearly latitudes are about 22° and 8°. The formation of high latitude spots near a sunspot minimum heralds the commencement of a new cycle of spot activity.

In the case of solar prominences, their appearance waxes and wanes very closely with the sunspots, as is indicated in the second curve of Fig. 3. When there are numerous spots there are many prominences, and vice versa. There is, however, a very big difference between their behaviour as regards solar latitude. Prominences can occur in any part of the sun, and they can be as large and conspicuous at the poles as they may be at the equator.

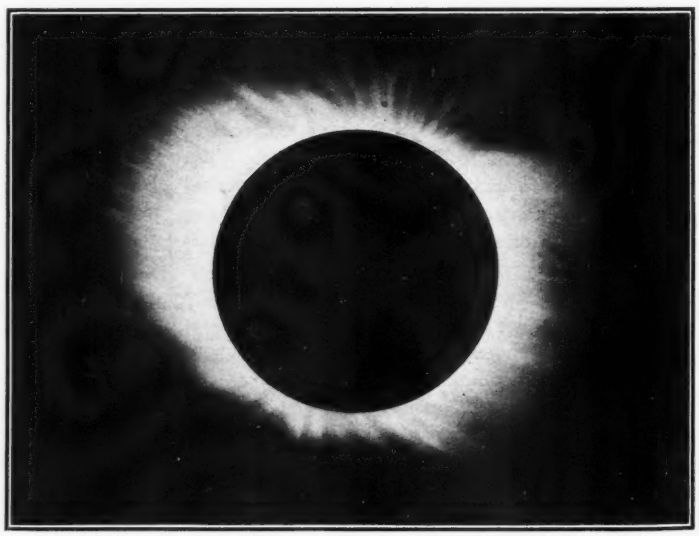

FIG. 2.—Solar corona, May 28, 1900. Minimum type. Exposure 30 sec. By E. E. Barnard.

To illustrate the striking difference between the extent of latitudes covered by spots and promi-

nences, the accompanying diagram (Fig. 4) has been made showing this distribution for the years 1892, 1893, and 1894, the year 1893 being a year of sunspot maximum.

The central vertical line represents the solar equator or latitude 0°, and the black areas on each side of it indicate the areas of prominences for each of the three years (all drawn to the same scale) plotted for every five-degree zone of latitude on the scale given at the bottom for both north and south solar latitudes. It will be seen that prominences are distributed in all solar latitudes in these years, and in some cases are very pronounced in high solar latitudes in both hemispheres, as indicated by the pronounced peaks in the area curves.

The smaller white areas represent the areas of sunspots for the same years. They are all drawn to the same horizontal scale as regards latitude distribution as the prominences, but the vertical scale for their areas, while the same amongst themselves, is different from that of the prominences.

The diagram clearly shows, in the first place, the insignificant distribution of the spots in latitude compared with that of the prominences, and in the second place, that the zones of maximum spotted area, in these years about sunspot maximum, lie within 20° of latitude on either side of the equator.

A study of the fourth series of curves in Fig. 3 shows that when prominences are at their maximum frequency they occur in two zones, the mean yearly latitudes of which are about 70° and 25°. When they are fewest in number their mean yearly latitude is about 40°. This cyclical change of latitude from year to year is shown in the diagram.

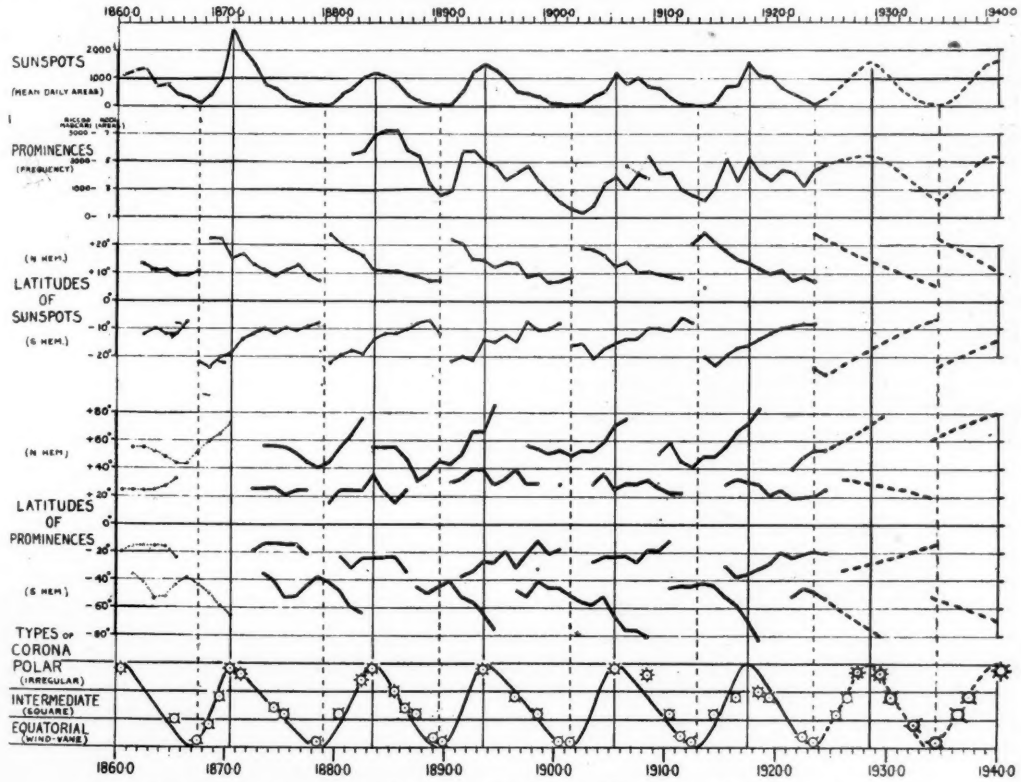


FIG. 3.—Diagram showing the relationship between the different forms of the corona and the latitudes of prominences.

Thus while the epochs of sunspot and prominence maxima are practically coincident, the spots at these times are most active in latitude about 18°, while prominences exhibit their greatest activity in about latitudes 70° and 25°.

Now from the prominence curves for both hemispheres it is quite easy at a glance to distinguish the years when these phenomena attain high latitudes, such as 60° or more. The question is, do these years pick up those cases in which the corona has been observed, and described as of 'polar,' 'irregular,' or 'maximum' type?

To answer this question easily, the various forms

of the corona, as recorded by different eclipse observers since the time of routine prominence observations first began, namely, in the year 1872, are brought together at the bottom of Fig. 3. Thus, for example, all the forms termed 'polar' are placed in the first horizontal strip in their respective years of observation according to the time-scale indicated at the bottom. All those designated 'intermediate' and 'equatorial' are also placed in lower strips at their observed years. It is found that a curve can be drawn through them

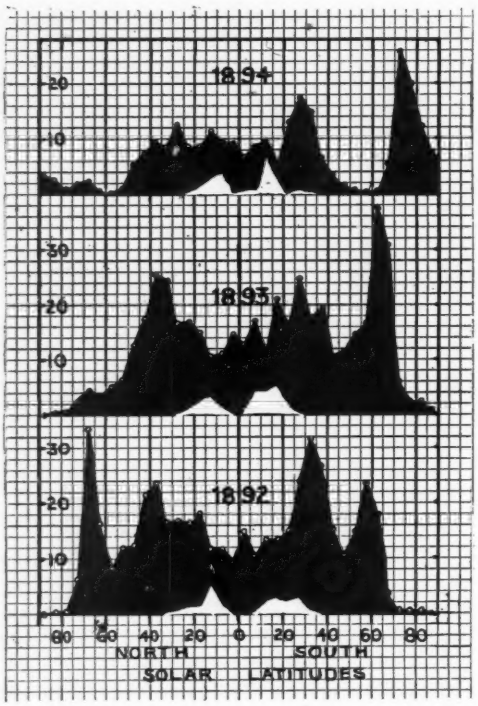


FIG. 4.—Diagram showing the distribution in latitude of prominences (dark areas) and sunspots (light areas) for the years 1892–94 (sunspot maximum 1893).

approximately resembling the sunspot and prominence curves at the top of the figure. Thus it is seen that the forms of the corona follow one another in the following sequence, namely, polar, intermediate, equatorial, intermediate, polar, etc.

Comparing these coronal types with the curves immediately above them, it is fairly obvious that 'polar' forms are coincident with 'high latitude' prominences in every case except one, namely, in the year 1883. A re-examination of the prominence data for that year discloses the fact that their presence was recorded up to latitude 65° in the northern and 75° in the southern hemisphere; the yearly curve did not, however, exhibit distinctive peaks in either hemisphere at those latitudes, but only a gradual reduction in area from the prominent peaks in latitudes 25° north and south. There were therefore high latitude prominences in that year, so that 1883 is no longer an exception as expressed above.

It is important to point out further that there is no case of an 'equatorial' or 'intermediate' form of corona being recorded when the prominences were in high latitudes.

The deduction to be made is, therefore, that it is only when prominences are near the solar poles that coronal streamers will be found there. This fact points clearly to the conclusion that prominences are the prime factors in the formation of coronal streamers.

Limitations of space permit one only to mention here the fact that there is a mass of other evidence which can be brought to bear to show the close association of prominences with coronal forms, as, for example, the arch-forms of coronal matter so often recorded as being directly situated over large prominences, in spite of the fact that the material of which prominences are formed is quite distinct from coronal matter.

A more detailed study of the diagram shows that while prominences near the solar poles are responsible for the 'polar' or 'irregular' types of the corona, the 'intermediate' forms owe their origin to the presence of two zones of prominences, and the 'equatorial' forms to one zone in each hemisphere.

The rhythmic nature of all the curves, as shown by the continuous lines, suggests that they can be continued for a few years without any great error. This has been done in Fig. 3 by means of broken lines[1] up to the year 1940, and below them are forecast the various forms of the corona which may be expected in future eclipses up to that year, based principally on the probable prominence curves. The corona of the present year should therefore be 'polar' or 'irregular,' corresponding to a very disturbed state of the sun's atmosphere. It should thus be irregular in form, devoid of pronounced polar rifts, and should be very bright. It may exhibit long streamers in any solar latitude. Its brilliancy will probably prevent any but the brightest stars from being seen, but on the other hand it lends itself very favourably to special studies of its composition by means of the spectroscope.

[1] There have just been published the latitudes of the mean areas of prominences for the first half of the year 1926. This shows that the zones of maximum area lie at latitudes 40° and 70° in the northern and 38° and 60° in the southern hemisphere, thus closely endorsing the points forecast in Fig. 3 for that year.

## The Moon's Shadow in Relation to the Earth on June 29.

By Dr. E. H. Rayner.

THE shadow of the moon, considered as a slightly conical cylinder about 240,000 miles long, intersects the earth at Cardigan Bay at $5^h\ 23^m$ G.M.T. At this moment the imaginary point of the shadow is about 1500 miles within the earth. The average diameter of the shadow as it crosses England is about 15 miles where it intersects the earth, being the value of the minor axis of the elliptical area of the earth's surface within which the eclipse is total at any moment. The altitude of the sun being about 11°, the major axis of the ellipse is four to five times as long as the minor axis. The azimuth of the sun is some 20° to the south of the direction of the shadow track; and this, combined with the elliptical form, causes the width of the track of totality to be about twice the diameter of the shadow cone.

The accompanying illustration (Fig. 1) shows a model of the conical shadow at $5^h\ 23^m$. The scale of the model is 10 miles to the inch, which is that of the special Ordnance Survey Map. It will be seen that the centre line of the shadow crosses the east coast of England over south Yorkshire at a height of about 45 miles. An important height from the point of view of radio transmission is about 100 kilometres, or 60 miles. The intersection of the centre of the shadow with this level has therefore passed over England towards the east before the intersection at the earth's surface has arrived at Wales from the south-west. The plan position of the intersection at a height of 60 miles has been specially computed by Dr. L. J. Comrie on behalf of the Radio Research Board, to whom I am indebted for much of the information of a numerical character.

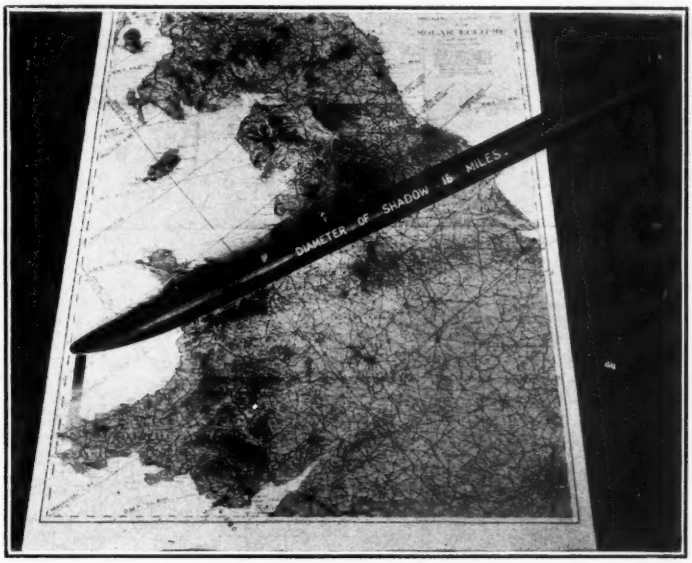


Fig. 1.—Three-dimensional model of shadow of the moon near the earth's surface.

Fig. 2.—Position and orientation of England during the eclipse.

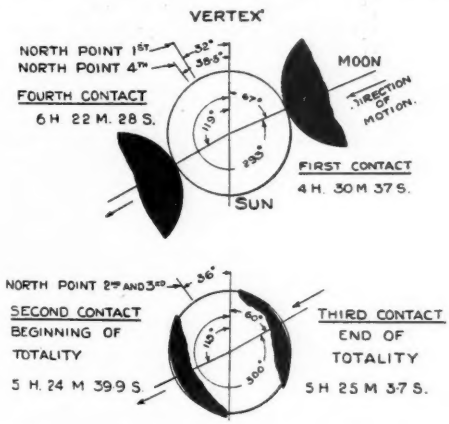


Fig. 3.—Circumstances of the eclipse for Richmond, Yorkshire.

Over England the plan position of the intersection at 60 miles high runs nearly parallel to the 0·98 totality line to the south of the central track. It is for most of the distance between the Bristol Channel and the North Sea, within 2 to 5 miles of this line and to the south of it. The location at $5^h\ 20^m$ is shown in the lower margin of the illustration, and that at $5^h\ 22^m$ is near the east coast. It is interesting to note that according to the data used in the computation, the centre of the shadow has not reached the earth's surface at $5^h\ 20^m$, its nominal time of contact being $5^h\ 20{\cdot}1^m$.

The season and the early time of day at which the eclipse takes place, being before 6 A.M. by solar time for first, second, and third contacts, causes the lines indicating simultaneous maximum phase on the map to be displaced more than 90° from the lines of longitude to which they approximate later in the day where the eclipse takes place at local noon. The map (Fig. 2) shows the shape of England as viewed in the direction of the shadow axis, the plane containing the earth's axis being imagined to indicate the vertical.

Arrangements have been made for the party organised by the Physical Society of London to have the use of accommodation at the Race Course at Richmond. The times of the contacts and position angles are shown in Fig. 3. The computation has been made taking into account the corrections given by Dr. Comrie, as a result of the information recently provided by the Astronomer Royal. The difference between the position angles of second and third contacts, 185°, being larger than 180°, shows that the point is a few hundred yards to the north of the centre line.

## Astronomical Programmes of Eclipse Work.

By F. J. M. Stratton.

ALTHOUGH the Joint Permanent Eclipse Committee has organised no special expedition to observe the eclipse of June 29, it has arranged that all instruments in its charge should be employed at the eclipse and has acted as an intermediary in securing instruments and local facilities for various expeditions. Ready co-operation has been invariably met with locally.

The first eclipse camp on the track of which the programme details are available is at Llysfaen Head, near Colwyn Bay. Here Dr. R. J. Clark, of the University of Edinburgh, Prof. H. Dingle, of the Imperial College of Science and Technology, South Kensington, and a group of research students from the University of Cambridge, will be stationed. A 16-inch cœlostat will be used to feed two spectrographs—a slit spectrograph working with a very dense flint Parsons prism, and an objective grating-spectrograph using a parabolic grating lent by Mr. Merfield of Melbourne, especially ruled to be strong in the infra-red of the first order and the ultra-violet of the second order. Abney plates will be used, and it is hoped to extend the spectrum of the chromosphere, and if possible of the corona, well beyond $\lambda$10,000. Wireless experiments on the strength of signals will also be carried out. Prof. Nolan from Dublin will also be at Colwyn Bay studying atmospheric electricity during the eclipse, and possibly President Morehouse from Des Moines University, Iowa, may be there with a spectroscopic programme.

Where the eclipse track strikes land again at Southport, the Oxford expedition under Prof. H. H. Turner and Dr. H. Knox-Shaw will be stationed. Their programme consists of direct photography of the corona together with photographs in polarised light. At Stonyhurst, Father O'Connor will also be taking direct photographs of the corona and flash spectra, working so far as possible into the infra-red. It is understood that Prof. da Costa Lobo, Director of the Coimbra Observatory, Portugal, will be at Stonyhurst with a cinema camera and spectroscopic apparatus.

The expedition from the Royal Observatory, Greenwich, will be in the grounds of Giggleswick School, near Settle. Its programme consists of direct photographs of the corona with a 6-inch lens of 45 ft. focal length, the spectrum of the chromosphere in the extreme red and a comparison of the intensities and heights of H and K and of the $Ca^+$ triplet $^2D - {}^2F$ at $\lambda\lambda$ 8498, 8542, 8662.

Mr. Evershed will be at Tunstall, about five miles from the central line, working with a large prismatic camera with two 6-inch prisms of 45° angle and a lens of 21 ft. focus. He will be studying the cusp spectrum before and after totality and the flash spectrum, and will be largely concerned with the height of the various gases. The Norman Lockyer Observatory expedition will be between Gilling and Richmond with a programme of direct photographs of the corona and small-scale spectra. Other observers who are expected to carry out observations in the belt of totality in Great Britain include Mr. Albert Taylor at Llanbedrog, Prof. E. A. Owen at Bangor, Mr. Harold Thomson and Mr. F. Sargent, of the Observatory of Durham, at Catterick, Dr. F. W. Aston and Mr. Wilfrid Hall also in Yorkshire. Prof. A. Fowler at the Imperial College of Science and Technology, London, and Prof. R. A. Sampson at Edinburgh, are proposing to study the cusp spectrum with high dispersion spectrographs during a considerable interval on either side of the time of maximum eclipse.

The other British expedition is that of the Solar Physics Observatory, University of Cambridge, which proceeds under Prof. H. F. Newall to Aal, in the Hallingdal in Norway. In addition to direct

photographs of the corona the programme includes spectroscopic observation of the chromosphere in the ultra-violet and the infra-red, with special application to the relative intensities of the red calcium triplet and of H and K. The intensities of lines at different heights will also be studied. Spectrophotometric work will figure largely in the programme, and there will also be polariscopic observations and interferometer photographs of the corona.

The American expeditions are all, except that of President Morehouse, to be stationed in Scandinavia. Prof. H. T. Stetson, of Harvard Observatory, and Mr. Weld Arnold, will be doing photometric work on the total brightness of the corona at Fagernas, where Prof. S. A. Mitchell, of the Leander McCormick Observatory, will also be stationed. Farther north, possibly near Gellivare, will be the Dutch expedition, including Dr. Minnaert, with the spectrophotometric programme which they were prevented by clouds from carrying out in Sumatra last year. Dr. Luyten will accompany this party. In addition to Prof. Stetson's photometer and the Dutch instruments, it is understood that Dr. Anderson's eclipse spectrograph, which was also blocked by bad observing conditions in Sumatra, will again be brought into use, in the hands of Dr. S. Rosseland. Finally, an Italian expedition under Prof. Horn d'Arturo may be stationed in Norway, and it may be assumed that there will be several Scandinavian expeditions and German expeditions in Scandinavia also. Details of their plans have not come to hand.

Nowhere along the track can it be said that weather prospects are very good. On the other hand, they are not so bad that we may not expect fairly confidently results of value from some of the many expeditions that will be observing the eclipse.

## Observing Parties and Stations.

### The Norman Lockyer Observatory.

DR. W. J. S. LOCKYER, director of the Norman Lockyer Observatory, informs us that the expedition from the Observatory will occupy a site on the eastward and adjoining side of a good road connecting Richmond with Gilling village, and distant one mile from the market place of the former town. It is on the Marquess of Zetland's estate, and he has very kindly given permission for the Observatory's party to take up its position there. The site is situated on a 600-foot contour line, and looking away from it towards the north, through the east and to the south, the ground falls away sharply at first and gradually afterwards. In fact, there is no land reaching anywhere near 600 feet within twenty miles of a line drawn due east and twenty-five miles a little north of east.

The programme of work to be attempted at Richmond is to secure large- and small-scale photographs of the corona, chromosphere, and prominences, and also a photograph of the spectrum of the corona.

The conditions of the eclipse are not sufficiently favourable to warrant the transport of a large spectrograph for photographing the chromosphere at second and third contacts. As such photographs can be obtained at stations where the partial phase is very large, arrangements have been made to accomplish this at the Sidmouth Observatory, where a large spectrograph is available.

Time determinations of the contacts will also be attempted, use being made of the broadcast time signals for checking the rate of the chronometer. The large-scale photographs of the corona will be taken with a camera the lens aperture of which is 6 inches, having a focal length of 31·5 feet, giving a solar image 3·3 inches in diameter. This instrument will be rigidly oriented to the eclipsed sun, the photographic plate only being moved by mechanism. One exposure only will be made.

Two smaller coronagraphs will in addition be mechanically driven, one having a 6-inch aperture and 4 feet focal length and mounted equatorially, and another a $3\frac{1}{4}$ inch Dallmeyer rectilinear lens of 15 inch focal length mounted on a 12-inch siderostat. One plate will be exposed in the latter and two or more in the former.

Four or more other coronagraphs from 4 inches aperture or less, including a Zeiss triplet aeroplane lens working at $f$ 4·8 and an Aldis triplet aeroplane lens working at $f$ 5·6, will be fixed in position, and two or more plates will be exposed in each, with exposures of two seconds or less.

The large-scale photographs of the chromosphere and prominences will be taken with a 30-foot coronagraph, giving an image of about 3 inches. This instrument will be fixed in the direction of the eclipsed sun, and the exposures given will be very short. Altogether about eight exposures will be attempted.

To secure the spectrum of the corona on a small scale, a Thorp transparent grating mounted in front of a Zeiss triplet aeroplane lens working at $f$ 4·8 will be used. This instrument is mounted on the upper end of the polar axis of a 12-inch siderostat and is clock-driven. One exposure only will be made of the coronal spectrum in the first order of the grating.

The party from the Norman Lockyer Observatory will consist of Lieut.-Col. Sir Francis McClean, Capt. W. N. McClean, and Dr. W. J. S. Lockyer, but much additional volunteer assistance will also be available to manipulate several of the smaller instruments and undertake other observations.

### Royal Observatory, Greenwich.

THE headmaster of Giggleswick School has kindly placed a field at the disposal of the Greenwich expedition. The observing party will consist of the Astronomer Royal, Dr. Jackson, Mr. Davidson, Mr. Melotte, and Mr. Woodman. Three different observations will be attempted.

(1) A large-scale photograph of the corona will be taken. For this a 6-inch lens of 45 ft. focus,

kindly lent by Mr. Worthington, will be used. This gives an image of the sun 5 inches in diameter. There is only time for one exposure of approximately 20 seconds.

(2) With a spectroscope of Littrow type, using a prism of 40° angle and 7 inches in height and a lens of 11½ ft. focus, the spectrum of the chromosphere in the red and infra-red will be photographed. Plates dyed with dicyanin will be used, and the spectrum will extend from the magnesium triplet in the green well into the infra-red.

(3) Prof. Milne has shown the importance of a photometric comparison of the H and K lines in the chromosphere; that is, the doublet $1^2S - 1^2P$ of ionised calcium, with the calcium lines at λ8498, 8542, and 8662; that is, the doublet $1^2D - 1^2P$ with its satellite. A spectroscope of reflecting Littrow type has been arranged to give these lines on the same plate. Neocyanin plates recently introduced by the Kodak Company will be employed. By the use of a coarse grating on the solar spectrum the intensity scale will be determined for different parts of the spectrum.

The times of beginning and end of totality will be observed with a direct-vision spectroscope on a 3-inch telescope.

### University of Oxford.

The expedition to observe the eclipse at Southport, on the cordial invitation of the Mayor and Corporation, will be a joint expedition from the University and Radcliffe Observatories, Oxford, arranged by Prof. H. H. Turner and Dr. H. Knox-Shaw in collaboration.

The astrographic object-glass of the University Observatory, fitted to a temporary mounting used previously in Egypt in 1905, and pointed to the 16-inch cœlostat, recently purchased from the Royal Society, will be used to take two or three photographs of the corona. (The purchase of the cœlostat, and its renovation, were rendered possible by timely grant from the trustees of Lord Leigh's Fund.) In addition, two photographs will be taken in light polarised in planes at right angles by means of reflection from plane glass plates blackened on the back: the lenses are the similar components which made up the Abney lens used in many eclipse expeditions, and the reflecting plates will be placed near and within the focus. These two cameras will be mounted equatorially.

During a preliminary visit last January, when the kind help and advice of Mr. C. Davidson, of the Royal Observatory, Greenwich, were available, the site for the observations was fixed in the grounds of King George V. School, on the invitation of the headmaster, Mr. G. A. Millward, who has also extended his hospitality to 100 boys who will come from London under the care of the *Daily News*.

### Stonyhurst College.

At Stonyhurst College arrangements are being made by Father O'Connor, director of the Observatory there, to take direct photographs of the corona with the 4-inch coronagraph lens belonging to the Royal Irish Academy, of 19 ft. focal length, a 6-inch Dallmeyer portrait lens of about 30 inches focal length, and a Dallmeyer telephoto lens.

An attempt will be made to photograph the corona with the 15-inch equatorial, using a green screen with maximum transparency at λ5300.

Flash spectra, as far into the red as possible, with the 19-ft. lens used as a prismatic camera, will also be attempted.

With the Hilger spectrograph, work will be carried out on the cusps before and after totality.

Quick runs on the magnetographs, and meteorological observations during the course of the eclipse, will complete the Stonyhurst official programme.

### Solar Physics Observatory, Cambridge.

The altitude of the sun will not exceed 13° in any site in England at the time of the total phase, and accordingly the conditions in Norway were investigated. It was found that the altitude will be nearly twice as great in Hallingdal and the neighbouring valleys, and the local conditions thereabouts were examined. Prof. Newall and Mr. Carroll went over to Norway in September for three days, and had the great advantage of enlisting the kind help of Prof. V. Bjerknes, who met them at Aal (Hallingdal) on the railway between Bergen and Oslo. Here, after considerable exploration, a site was found in all respects convenient, though, as had been anticipated, the difficulty in that rocky and wooded region was to find a level site which was not already occupied by a house or a farmstead.

The observations which will be aimed at are the following:

Spectroscopic observations of the chromosphere in the ultra-violet (Prof. Newall). Spectroscopic observations of the red calcium lines in the chromosphere, high dispersion, and of both red and violet calcium lines in the chromosphere, low dispersion (Mr. Carroll and Mr. Stratton). Spectroscopic observations with objective grating and moving photographic plate (Mr. Butler). Photometric work in connexion with each of the above. Interferometer observations of the corona (Prof. C. Bryant). Large-scale coronagraph photographs (Mr. W. M. Smart). Polariscopic observations of the corona (Prof. Newall).

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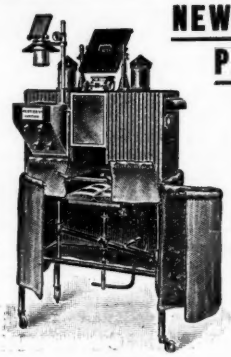

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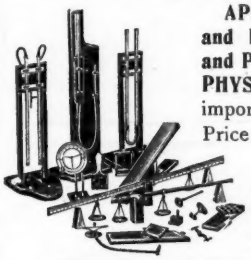

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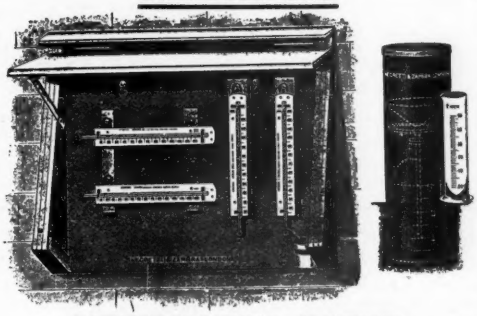

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A new volume of NATURE—the 120th—will begin on July 2. That number will therefore be a good one with which to start a subscription.

The issue will contain an addition to the "Scientific Worthies" series and a separate photogravure plate portrait of the subject of the article—Prof. R. Willstätter.

In addition, there will be a Supplement consisting of the Royal Institution address of Sir Humphry Rolleston, Bart., entitled "Concerning Old Age."

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Preference given, other things being equal, to ex-Service men. The post will carry superannuation under the Federated Universities Superannuation System : Government contribution being 10 per cent. of mean of scale, individual contribution 5 per cent.

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Application should be made on a form to be obtained from the CHIEF SUPERINTENDENT, Royal Aircraft Establishment, South Farnborough, Hants, to whom it should be returned, quoting reference No. A.180, not later than July 16, 1927.

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Further information, including particulars as to salary, together with the necessary form of application, may be obtained from the SECRETARY, Department of Scientific and Industrial Research, 16 Old Queen Street, S.W.1. Completed forms of application must be returned not later than July 11.

## DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH.

A RESEARCH PHYSICIST is required at the Building Research Station, Garston, Watford. Candidates should possess a good Honours degree in Physics with some research experience. Scale of salary £175-£15-£235 plus Civil Service Bonus; total initial pay £278 : 19s. Superannuation under the Federated Superannuation System for Universities. Preference given, other things being equal, to ex-Service men. Forms of application can be obtained from the SECRETARY, Department of Scientific and Industrial Research, 16 Old Queen Street, S.W.1, to whom they must be returned not later than July 11.

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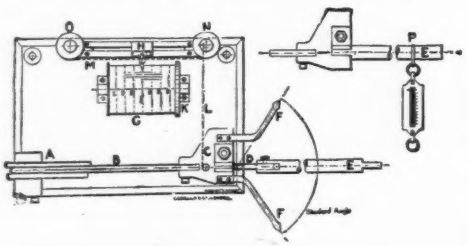

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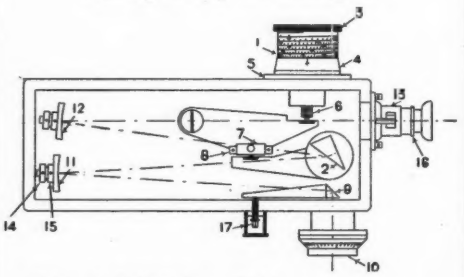

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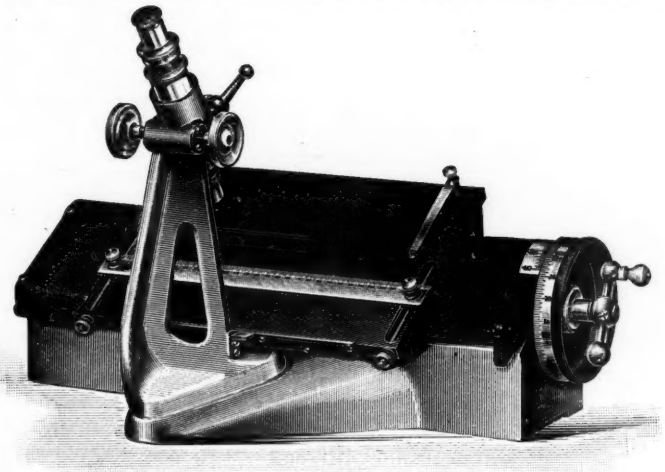

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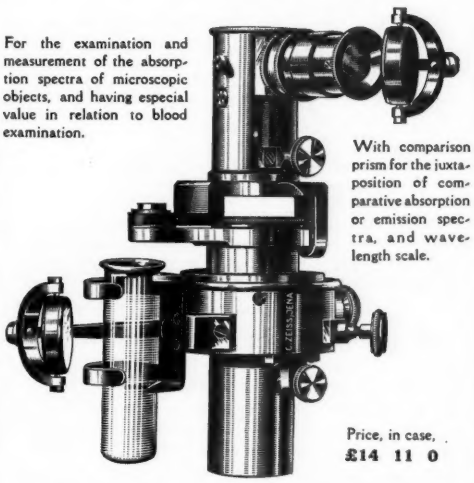

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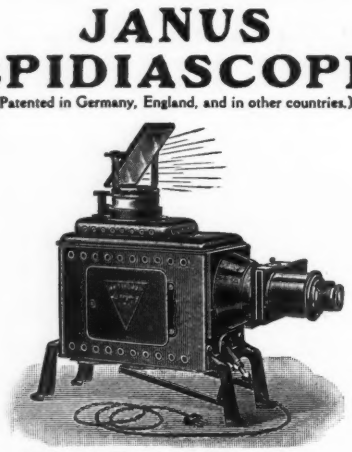
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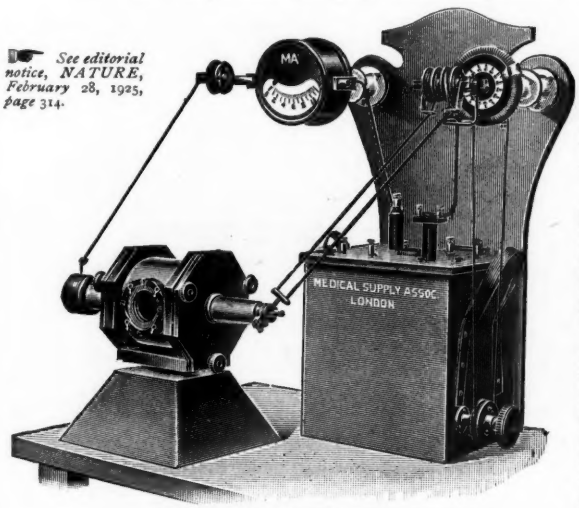


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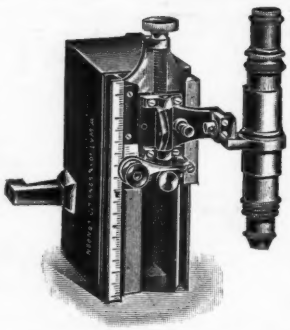

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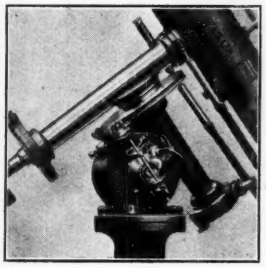

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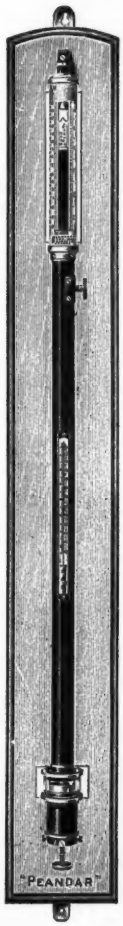


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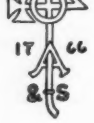


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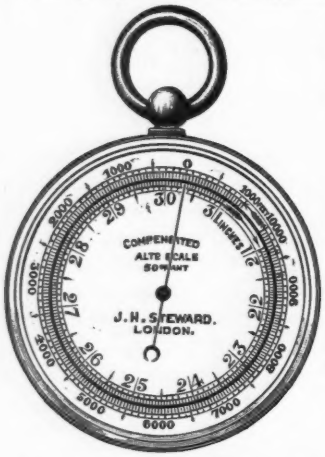


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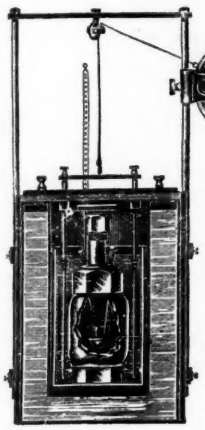

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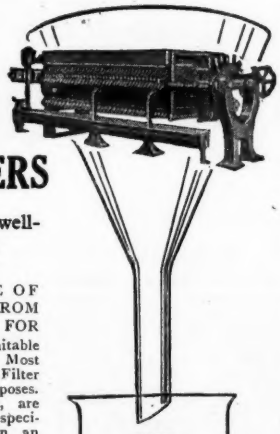

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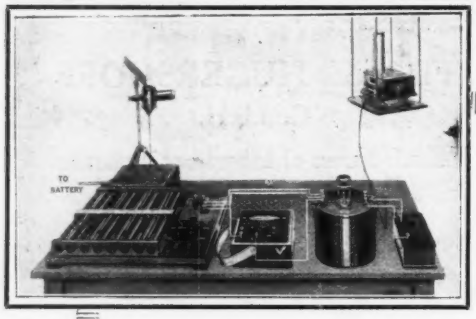


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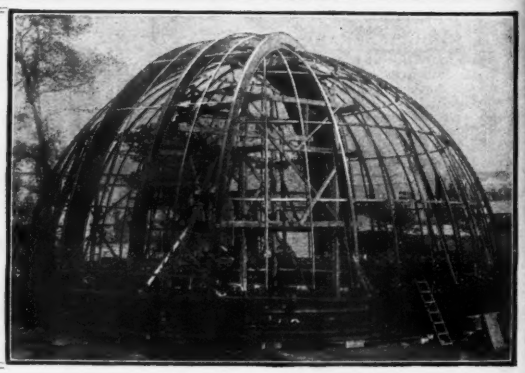


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